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CONTENTS

The Practical Prospects for Disarmament and Genuine Peace. GRENVILLE CLARK 645

The Origin and Persistence of Standards of Professional Conduct in the Practice of Law. HENRY S. DRINKER 652

Studies of the Earth's Crust Using Waves from Explosions. H. E. TATEL, L. H. ADAMS, and M. A. TUVE 658

The Significance and Technical Analysis of Ancient Textiles as Historical Documents. RENÉ BATIGNE and LOUISA BELLINGER 670

The Defects in Crystals. C. G. SUITS 681

Photographic Aids and their Uses in Problems of Authenticity in the Field of Paintings. HENRI MARCEAU 686

Index to Volume 97. 713

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THE PRACTICAL PROSPECTS FOR DISARMAMENT AND GENUINE PEACE

GRENVILLE CLARK

(Dinner Address, April 24, 1953)

I NEED hardly tell you how much I value your invitation and the opportunity to meet with this famous Society. I am especially grateful for the generous introduction of your President, Mr. Justice Roberts, whom I have long respected and admired.

I take as my subject: "The Practical Prospects for Disarmament and Genuine Peace."

Is genuine peace in an ordered world a mere wishful dream or is it now a practical prospect which practical men can work for with reasonable hopes? I shall express a tempered optimism that through disarmament under a drastically revised United Nations, a world governed by law *will* emerge from the present crisis—not at some remote date, but within some such period as ten to twenty years. I hope to present sound reasons why we can justifiably look forward to such an outcome, instead of resigning ourselves either to a new catastrophe or to an indefinite period of strain and tension.

I choose this subject for several reasons. One is that it is a field of study which has long preoccupied me. Another is that the problem of preventing war is probably the most vital subject (in a literal sense) now before the world. Beyond that is the fact that this question of keeping the nations, as has been said, from murdering each other is perhaps the most baffling of all human problems.

You will recall the observation of Professor Einstein on that point. When asked how it is that when the mind of man has been able to solve the secrets of atomic fission, we have made so little progress in controlling modern means of destruction, he replied: "The answer is simple, my friend. It is because politics is more difficult than physics." How true! But I shall submit that, under the pressure of events, the light is dawning so that now at last there is a reasonable prospect for an ordered world.

I have still another reason for my subject, namely, that there could be no more appropriate theme at a meeting of a Society founded by Benjamin Franklin. There are many evidences that in the vast range of Franklin's interests, the subject of peace was one of the foremost. But none is more striking than Franklin's interest in the "Project of Perpetual Peace" of the humble schoolteacher Pierre-André Gargaz, which Franklin had printed at his private press at Passy, in 1782. Through the kindness of one of your members, Professor Julian P. Boyd, I recently received a reprint of the fascinating documents relating to this plan, some of which are in your archives. If Franklin was concerned with the problem of peace in 1782, would he not have been all the more occupied with it if he were with us here 171 years later?

At the outset I stress my view that disarmament is the crux of the problem of genuine peace; and that progress toward the goal will be measured by the extent to which this basic fact is recognized.

There is no mystery in the proposition that in this age genuine peace, as distinguished from an armed truce, is impossible while the nations remain armed.

At the present moment, there are in the world some 20 million men in active military service. The annual cost is some 100 billion dollars, which represents about one-ninth of the world's gross product. There is no pause in the creation of new and appalling weapons. In these conditions, mutual fears and tensions are naturally so great as to render negotiation on specific issues almost impossible.

Beyond this the drain of the arms race on the world's energy and resources is so great as to make adequate aid to the underdeveloped areas of the world virtually impossible. And yet the state of poverty in which nearly half of the human race now lives is recognized as an underlying cause of unrest and war.

It follows that, in the effort for world order, the adoption of specific and effective plans for disarmament must be the fundamental feature. All else, as I contend, must be subordinate to and dependent upon the achievement of disarmament.

Is it surprising that in the tense and suspicious atmosphere of a world armed with modern weapons it is a virtual impossibility to reach even nominal settlements on important questions? But if nominal settlements of the principal political issues were achieved—such as the future of Germany and Eastern Europe, the unification of Korea and the status of Communist China—it might profit little if the nations continued to confront each other heavily armed and without adequate international machinery to enforce the so-called settlement. In that event, the atmosphere of distrust would continue; and genuine peace would be almost as far away.

Does it not follow that in the current effort towards peace, the adoption of effective plans for disarmament must be *put in the forefront;* and that settlements on specific issues can probably come only as a result flowing from disarmament rather than as preliminary to it?

In fact we are only trifling, I believe, with the problem of genuine peace until we realize that disarmament is a pre-condition.

Consider the effect of the arms race on the settlement of specific issues.

Take, for example, the "liberation" of Eastern Europe. Is there any actual chance, short of the internal disintegration of the Kremlin regime or its defeat in war, that Russia would in an *armed* world consent to relinquish its domination of Poland, Czechoslovakia, and Hungary? With bitter memories of successive invasions from the West, is it not certain that—short of disintegration or defeat—Russia would wish to hold control over these buffer lands? But in a *disarmed* world, why should it not be possible to agree upon restoration of the full independence of those nations *concurrently* with the *actual carrying out* of an effective plan for universal disarmament?

Take Korea. In view of the threat to Japan, could the West in an *armed* world afford the risk that a united Korea might come under Soviet-Chinese control? Or by the same token, could Russia and Communist China risk Korea's domination by the West? But in a *disarmed* world, when Korea could be a threat to no one, why should there be any serious obstacle to Korea's unity and independence?

Such problems also as the future of Germany, the control of the Turkish Straits, the future of Indo-China and Formosa could readily yield to negotiation when a great disarmament plan has been agreed upon and put into at least partial operation, whereas these and other questions seem virtually insoluble in a tense, armed world.

In short, it is time to recognize that any real settlement of the world's most vital political questions is impracticable while the arms race continues; and that important political settlements and disarmament are inextricably intertwined.

Holding this conviction, I view with dismay the interpretation put on President Eisenhower's proposals of April 16 that some important settlements, such as the future of Germany and Korea, must be arrived at *before* a disarmament plan is agreed upon and applied. If this was the intention, I venture that the order of priorities is erroneous and that his proposals will prove sterile.

It is therefore to be hoped that the President's proposals will be otherwise construed, i.e., that all important political settlements will be sought *as a consequence* of disarmament rather than as something which should or can precede an accord upon a comprehensive disarmament plan. I regard the acceptance of this basic conception as the key to success in the struggle for world peace, not only in the long run but in the current effort.

At this point I wish to make it crystal clear that I do not suggest unilateral disarmament by the United States or anyone else. All must disarm or none will.

I make it clear also that the recognition of disarmament as indispensable to genuine peace implies no disapproval of the current rearmament effort. In our present anarchic world of sovereign states, with no world law or world police, there is no *immediate* alternative to military strength. Yet we must never forget that however necessary our present rearmament policy, that policy embodies no element of world order under law. On the contrary, it is the essence of power politics. Granting that rearmament has been helpful in gaining time to seek the solution it is in itself no stable solution at all. With entire consistency, therefore, we can support immediate rearmament and simultaneously work for universal disarmament.

I ask you to understand also that when I speak of disarmament, I mean disarmament that is not only universal and enforceable, but also complete. I do not mean mere "control" or "reduction" of national armaments. I mean their *abolition*, right down to the level of strictly

limited and lightly armed forces for internal order only.

Everyone now perceives that disarmament must be proportional and universal. Everyone knows also that disarmament must be enforceable—"foolproof" as nearly as may be. The additional requirement—that disarmament must also be *complete*—is less generally understood, but should be. Not only is complete disarmament desirable; it is actually easier of achievement than any form of partial disarmament. For the record of history shows that efforts for mere "reduction" or "regulation" or "control" will almost certainly bog down in interminable disputes over quotas and the so-called "needs" of the Powers. This is common sense as well as the lesson of history.

It is, therefore, to be regretted that our President still seems to speak in terms of mere "control" or "reduction" of so-called conventional weapons, thus leaving it open to maintain powerful national forces even though the effort to abolish atomic weapons and other means of mass destruction were successful. With respect, I submit that if our leaders are to achieve real results, they must come to realize that partial disarmament is impracticable and must speak and act for the virtually *complete* disarmament of all the nations in all arms.

Now if you follow me in the conclusion that disarmament—universal, enforceable, and complete—is indispensable to genuine peace, many will say: "Why yes! But how and when can this be accomplished? Is this not a dream? At least is it not something to be realized, if at all, in an indefinitely remote future?"

This brings me to the heart of my subject. I want to submit some reasons why total disarmament is not only feasible but not unlikely of accomplishment within as short a time as twenty years.

The essential point lies in the rapid and terrific increase in the destructiveness of modern weapons. This increase proceeds on a scale and at a rate hardly yet realized. It creates a threat to all mankind of so unprecedented a nature as to create unprecedented pressures to find a solution. The seriousness of that threat is so great as to vitiate, in my view, the current talk that we may drift along for thirty or forty years in a state of tense armed truce. The colossal power and threat of the new weapons will, I believe, bring the situation to a head within a much shorter time. The world will then choose which road it will follow—whether toward a disarmed world organized under world law, or toward new and greater destruction.

I do not believe that the nations will take the latter road. I firmly believe that they will cease this intense arms race (although perhaps only by a close shave and just in time), and will universally agree to discard all their weapons under a regime of limited world law, administered by a revised United Nations.

As to timing I believe that the general agreement will come to pass within ten years and that the actual carrying out of complete disarmament —by all nations in all arms—will be well on its way to completion within twenty years. I believe this not because man will have suddenly changed his nature, but only because under modern conditions the renunciation of war will be found *expedient* as a matter of cold self-interest and even self-preservation.

The impelling cause will be the rapid, progressive, and unprecedented mastery over the means of self-destruction, already mentioned.

As to the power of the new weapons, I venture to remind you of some recent developments.

We are aware of them partly through solemn warnings from high officials. For example, on January 7, 1953 former President Truman told the Congress:

> Atomic science is in the full tide of development; the unfolding of the innermost secrets of matter is uninterrupted and irresistible. . . . From now on, man moves into a new era of destructive power, capable of creating explosions of a new order of magnitude, dwarfing the mushroom clouds of Hiroshima and Nagasaki.

On January 16, 1953 former Secretary for Air Finletter said: "The drive forward in atomic and other advanced weapons is real, intense and appalling." Only eight days ago on April 16 President Eisenhower spoke of the development of "weapons of war now capable of inflicting instant and terrible punishment upon any aggressor."

Equally important are the more definite surmises we can make as to the quantity and power of the new means of destruction. For good or bad reasons (mostly bad, I believe) the military keep the people in as much ignorance as possible on this subject. Yet we are not helpless in making estimates sufficiently accurate to serve as a guide to conduct. We can do so by putting together bits and pieces of public information

and by studying the comments of those who, while they do not know the precise facts, are well-equipped to estimate the probabilities.

For example, the noted military critic, Mr. Hanson W. Baldwin, has recently written that "the era of atomic weapons by the thousands is now at hand or fast approaching."

More concretely, the distinguished scientific correspondent of the *New York Times*, Mr. W. L. Laurence, and the Messrs. Alsop have ventured fairly definite estimates of our stock of atomic and thermo-nuclear weapons now on hand and our prospective stock eight years hence. Neither Mr. Laurence nor the Messrs. Alsop have had access to any classified information. But they have followed this subject for years so that, while their estimates are avowedly guesses, they are well-informed ones.

As to our present stock, the average of their estimates, which are quite close together, is that the United States now has the equivalent in explosive power of not less than 1,250 Hiroshima bombs. Since that bomb was the equivalent of 20 thousand tons of TNT, this means an explosive force equal to 25 million tons of TNT, or, in the new parlance, 25 megatons. This result has been accomplished in less than eight years since the first A-bomb explosion in 1945. They have, therefore, looked ahead another eight years through 1960. According to the average of their estimates, again close together, the United States will then have the equivalent of not less than 7,500 Hiroshima bombs, which means an explosive force of 150 million tons of TNT or 150 megatons.

Without some means of comparison these vast figures mean little. But if one remembers that the total bomb tonnage dropped on Germany in World War II was less than one and one-half million tons of TNT (approximately 1,400,000) one can get a slight idea of recent "progress" in destructive power. On this comparison, the estimated *present* stock of A and H-bombs in the United States alone would equal in explosive force almost eighteen times the total bomb tonnage dropped on Germany; and the *prospective* stock at the end of 1960 would be the equivalent in explosive power of more than one hundred times that total tonnage.

What can be said as to the *killing* power of these megatons of bombs, as distinguished from their *explosive* power? This estimate is obviously more difficult owing to uncertainties as to the effectiveness of defensive measures, the degree of diffusion of targets, the presence or absence of warnings and the like. Yet we may venture estimates that can give us some idea of the slaughter in which we would be involved once our military men were turned loose.

As to the bombing destruction in Germany in World War II, our official estimate is that with a dropped tonnage of about 1,400,000, the number of civilians killed was 305,000 with 780,000 wounded, or a total of nearly 1,100,000. This shows a ratio of one killed or wounded to each one and a quarter tons. On the same ratio, the dropping of let us say 100 megatons (100 million tons) would produce the killing or wounding of nearly 80 million persons, of whom over 20 million would be killed.

The results of the Hiroshima and Nagasaki A-bombs were, however, far more deadly per ton. Our official estimate of killed and missing in the two cities was 106,000 and of wounded 110,000, a total of 216,000. Taking the published figure of 20,000 tons of TNT as the approximate equivalent of each bomb (40 thousand tons for the two), we have a ratio for these A-bombs of one killed, missing, or wounded to less than one-fifth of a ton of TNT explosive power, or over six times the German ratio.

Between the German and the A-bomb results, it might be reasonable to take an average of two killed or wounded per ton or, say, 100 million dead or maimed from the dropping of 50 megatons of explosive power, representing only one-third of our prospective stock eight years from now.

Very few can say how far such figures are wide of the mark. They are as likely to be too low as too high. But whatever figures we choose to take, I surmise that if we cannot find a way to avoid atomic war, we, the American people, ought not to count on getting through with it without killing or maiming at least 100 million civilians, not to speak of the probable deaths of as many more from famine and hardship. It is needless to say that if this ever occurs we Americans would go down in history as the greatest mass killers so far recorded in the annals of man.

You can cut these estimates in half if you like and still have a picture of potential destruction in war wholly unprecedented in human history. One thing at least seems certain, namely, that, as Mr. Hanson Baldwin has put it, "the atomic

arms race is the greatest threat mankind has ever faced."

What are the inferences from these facts and estimates in respect of the practical prospects for disarmament and genuine peace?

One inference is, in my judgment, that this condition has stepped up the pace of history which marches at a sharper tempo than perhaps ever before. As already suggested, it means that we will probably come to the parting of the ways between disarmament under world law, on the one hand, and unprecedented destruction on the other, in a matter of years instead of centuries or even decades.

Another inference is that when the power of these weapons becomes more generally known, new and powerful motives for disarmament and a world of law will come into action.

What will be the nature of the paramount motives? In spite of the vast destruction of life and property that modern war would involve, I doubt whether mere fear will be a dominating motive. At least with us Americans, still somewhat protected by the oceans and possessed of the main power for mass killing, I suggest that an entirely different sort of sentiment will be superior. I mean a decent reluctance to take life and cause suffering on a great scale.

I am aware that we have gone a considerable distance towards callousness in war. When we even contemplate the possibility of the A-bombing of cities it is a far cry from the time only forty years ago when the sinking of merchant ships without warning and the bombing of civilians were thought beyond the pale. But the new weapons make a difference in degree great enough to be virtually a difference in kind. I venture, therefore, that there remains enough latent reluctance to kill and maim women, children and old men by thousands at a time to make the American people shrink from so vast a holocaust. If I am right a powerful weight will have been thrown into the scales for genuine peace.

In expressing my view of qualified optimism, I would not have you think that I minimize the obstacles. These include blind fear of change, excessive nationalism, and fear of impairment of "sovereignty." They include undue influence of the military in our own and other countries, stunted imagination, and limited vision. Add to these things, the mutual fears engendered by the arms race and the not yet ended barrage of accusation and insult between East and West—and you have indeed a formidable array of barriers.

I am aware, too, of the measure of truth in the cynical saying that "It is folly to expect men to do all that they can be reasonably expected to do." There is no positive assurance that man will not cripple or even destroy himself merely because it is plain that he will do this very thing unless he radically changes his ways.

And yet, in spite of all, I judge the weight of the factors making for disarmament and an organized world somewhat superior—especially the impulse of self-preservation and that of normal compassion for one's fellow man.

I believe this, however, on one important condition, namely, that more and closer thought is soon applied to the *concrete problems* of world organization. Benjamin Franklin observed that "The best public measures are seldom *adopted from previous wisdom but forc'd by the occasion.*" Clearly, however, he did not mean that the pressure of circumstances will automatically supply a solution. For he expressed a companion principle of equal rank—that people must be *shown* how to change their ways.

I invoke his dictum in a letter to Lord Kames in 1760:

> Many people lead bad lives that would gladly lead good ones, but do not know *how* to make the change. . . . To expect people to be good, to be just, to be temperate, etc., without *showing* them *how* they should *become* so seems like the ineffectual charity mentioned by the apostle which consists in saying to the hungry, the cold and the naked, "Be ye fed, be ye warmed, be ye clothed" without showing them how they should get food, fire or clothing.

How applicable to disarmament and the cessation of war! Nearly all the individual persons in all the nations *want* to quit killing each other. As the President has just said: "all humanity shares the common hunger for peace and fellowship and justice." Yet, as between whole peoples, the way has never yet been found to discard armaments and live in peace as we have learned to do in our towns, states, provinces, and nations. But now the peoples must really learn how to do so under penalties never before contemplated. This is why I stress the necessity for more intensive study of *the techniques* requisite for disarmament, together with a general public discussion as to what specifically needs to

be done by way of United Nations Charter revision.

In short, a creative minority must learn the subject and propose the necessary specific measures; and the majority must understand what is required sufficiently at least to acquiesce and approve.

In this way, we would have the combination which I believe Franklin would have envisaged as essential to a great new step: *Firstly*, a set of circumstances or "occasion" by which the reforms are "forc'd"; and *secondly* a public which knows through creative study and by instruction and discussion why the specific measures are needed to achieve the purpose.

There is certainly no difficulty about the pressure of urgent circumstances. They are right here today. And on the other point, I suggest that a better time is about to come in this country. With all our successes, we cannot fairly claim that the present period is one of bold political thinking in any way comparable, for example, to our Revolutionary period. Although there was never so great a need for pioneering political thought, I venture that there are not now among our 160 million people as many imaginative political thinkers as among the four million Americans of 1787.

It is not that we lack talent in general, but that too much talent of the first order has been drawn into pursuits far distant from creative politics in the world field. Now, however, I see a change coming in accordance with what seems the age-old law that under sufficient pressure men will be found to meet the need.

At the present time, I estimate that in our vast country, not over 100,000 people are actively engaged either in the study of world organization or even in its active support; and as to the number of excellent minds really devoted to the close study of world institutions, I believe that they can be counted on the fingers of two hands. I look forward, however, to the time within a decade when the number of our citizens actively engaged in such studies or supporting them will increase tenfold. I say this because in various other countries, which by hard experience know more of modern war than we, there is a much greater interest in world organization than among ourselves.

At the present time, for example, the number of people actively enrolled in the "One World" movement in Norway and Denmark is the equivalent of one million Americans, with every prospect that this number will increase. Why should we be so far behind them? I believe that the gap will be closed.

Two things are imperatively needed: *Firstly*, the formulation of detailed and concrete plans for universal, enforceable and complete disarmament and for the world institutions necessary to make disarmament effective; and *secondly*, a great campaign of public education throughout the world whereby millions may be brought to understand and choose between the various proposals.

To accomplish these things, our churches, our educational institutions, our women's clubs, our farm and labor organizations, our philanthropic foundations, and other great agencies of public education must come to life in this great field of effort. With a few exceptions, they have a long way to go, for they have hardly made a start in the realization of their opportunity and obligation. But here also I see hopeful signs.

One further thought: Let us not delude ourselves in the belief that we can look for salvation to our political leaders.

It is ancient truth that new dangers require radically new measures. Six hundred years ago Chaucer wrote: "New times demand new customs here below." Jefferson wrote that "institutions must advance to keep pace with the times"; and Abraham Lincoln said: "As our case is new, so we must think anew and act anew. We must disenthrall ourselves."

More than ever these maxims apply now. Yet there are inherent reasons why our public officials cannot *reasonably be expected* to initiate the novel and comprehensive measures that the situation requires. They are too much preoccupied with day-to-day problems, too inhibited by tradition, too apprehensive of being ahead of current opinion to be capable of the great steps forward in world organization that the atomic age requires.

It is conceivable that some statesman will emerge with both the capacity and the willingness to define and advocate the minimum world institutions that will alone suffice to make disarmament and genuine peace a reality. But this is unlikely. We will do better to assume the normal state of affairs and to realize that our salvation must come from the creative thinking and persistence of private citizens not subject

to the handicaps and inhibitions of the official person.

On the other hand, let us remember that, while the officials will almost certainly be incapable of *initiating* the new and drastic steps that the new world situation demands, they will readily *adopt* them if and when there is a sufficiently strong public opinion in their support. It follows that a great educational campaign is of crucial importance so that the public will the better and sooner understand what is required, including complete disarmament and drastic revision of the United Nations Charter.

I am very conscious that with so vast a subject, it has been out of the question to do more than make some observations on a few points which I deem of special importance. For if one were to try to weigh all the considerations providing encouragement on the one hand or discouragement on the other as to the practical prospects for disarmament and genuine peace, one would need not only the four hours allotted to Mr. Malenkov but also a "captive" audience with no choice but to stick it through. I can only hope that I may have stimulated your thought to some extent so that you may be induced to form part of that creative minority of which I have spoken.

In closing, permit me to summarize what I have tried to say.

Effective disarmament—universal, enforceable, and complete—is the key to genuine peace. Important political settlements can probably come only as a result of disarmament, rather than as preliminary conditions to disarmament. Hence a great effort for disarmament should have priority.

On the one hand, the world is engaged in an unprecedented arms race and faces an unparalleled threat. But that very fact makes us realize how great a slaughter we, the American people, would be led to perpetrate unless we find the way to an ordered world. This realization will bring to bear, I believe, enough decency and compassion to stimulate a more intense search for new means to avert the disaster. Beyond that there is promise of a new era of creative political thinking "forc'd," in Franklin's words, "by the occasion"; and in this promise there lies great hope.

We need also a great campaign of public discussion and education as to the specific world institutions requisite for disarmament and the rule of law in world affairs. We cannot rely on public men to *initiate* what is required; our salvation lies with ourselves. But here again is hope. For although there is much inertia and fatalism, it is also true that many capable men and women throughout the world are actively working for world order. Their number and influence will increase as the weapons become still more deadly.

On the whole I would leave with you a conviction of what I call a prudent hopefulness. To achieve world order under law, we must indeed have more creative political thought, more intelligence, and more pure political skill, in the highest sense. But these alone will not suffice. We shall need also a modicum of faith that we are not helpless to surmount our problem. Above all, we shall need patience and endurance to keep going in face of all obstacles.

It is not through any one of these, but by a combination of them all that the goal can be achieved. It will be achieved, I say again, not primarily by Presidents or Prime Ministers or Foreign Offices but by such people as meet here tonight in this renowned Society.

THE ORIGIN AND PERSISTENCE OF STANDARDS OF PROFESSIONAL CONDUCT IN THE PRACTICE OF LAW

HENRY S. DRINKER

Lawyer, Drinker, Biddle and Reath, Philadelphia; Chairman, Standing Committee on Professional Ethics, American Bar Association

(*Read April 24, 1953*)

AT THE TIME of the Norman conquest there was no class of professional advocates in England; nor did William bring with him any professional lawyers or written law or jurisprudence. In early times, however, the lord defended his men and the guardian represented his ward. The champion, in trial by battle, may be considered one of the ancestors of the legal profession. This mode of trial was probably brought over by the Normans, there being no allusion to it in the old Saxon laws. Apparently hired champions were forbidden in England, but in spite of this, much hiring, direct and indirect, seems to have gone on, the same champions reappearing and fighting again and again, each time on behalf of a different principal, and frequently not clear of one contract until they took on another. Bracton, who wrote about 1250, tells of one Elias Piggun who was found guilty by a jury of being a hired champion, to whom money had been paid to undertake the warranty, and who, as punishment "lost both his foot and his fist." Defendant in a trial by battle must have been ready to prove his innocence, either by defeating the plaintiff or his champion, by frightening him to default (as often happened), or by prolonging the "battel" through the day. If it lasted until the stars began to shine, defendant won the case.

Trial by combat was greatly to the advantage of the rich litigant, who was able to hire all the good champions. Glanvil, who died in 1190, welcomed trial by jury as an alternate to the duel.

Even before the conquest, however, it was the practice of litigants to take with them a friend as adviser—*consilium*. It is this adviser who is really the direct ancestor of the modern lawyer. During the next two hundred years this practice developed a class of legal advocates and pleaders, increasingly experienced and competent in legal technicalities, who eventually became the professional barristers. The early lawyers seem to have been mostly ecclesiastics, the only class which could read and write. About the year 1200 priests were forbidden to act as advocates in the courts, and it is said that those who had made the law their profession then assumed a close fitting headdress to hide the clerical tonsure. Some think that it was this headdress that became the wig of the modern English barrister.

Although by 1150 the law was as yet hardly a conscious profession, we have by then the definite names of persons who were conducting cases. While they could not sue for fees, a friend who had been of obvious help in the litigation expected and received a reward in the form of an honorarium. Before the destruction of Old St. Paul's Cathedral in the Great Fire, each serjeant had a pillar in the nave (like the brokers on the stock exchange) where he received his clients. In the modern barrister's gown there is a pucker at the back which is the relic of a little pouch in which the grateful client deposited his fee, theoretically without the lawyer's knowledge. A single streamer was attached to the pouch, by which the barrister, after the client had left, could pull it up over his shoulder and ascertain the extent of his client's gratitude.

In 1239 William of Drogheda, half priest and half lawyer, who possibly practiced in the secular as well as in the ecclesiastical courts, wrote a study on the trade of advocates, the aim of which was to teach his readers how to win their cases. He describes procedure, gives many practical suggestions (*cautelae*) including hints for rather sharp practice, and asks his readers to spend part of the fees resulting from his advice in masses for his soul. He was particularly explicit in his admonition to collect fees in advance. "Get your money," he says, "while the patient is ill."

By the time of Edward I at the end of the thirteenth century, we see that a legal profession

is being formed, consisting of two branches, the pleaders, who developed from the friend-adviser, (*consilium*) and finally became the barristers (*servientes ad legem* or serjeants-at-law), and the attorneys (*attornati*). The pleader did not originally represent the client, but was his counsellor, whose mistakes it was the client's privilege to disavow. The attorney, on the other hand, was more than a mere advocate, and represented the client's person. In the early days attorneys could be appointed only by permission of the crown, and it was not until 1235 that suitors were allowed generally to be represented by attorneys.

Apparently the earliest provision of law regulating the professional conduct of lawyers was the statute the First of Westminster, in 1275, which provided imprisonment and perpetual suspension from practice for any pleader who should deceive or beguile the court or his client. A London ordinance in 1280 laid down standards for admission and practice.

A pleader might not be on both sides of a case, or have a stake in the outcome; where appointed by the court to plead in a case he might not refuse without good cause and might not retire without his client's consent; the bullying of witnesses and parties was forbidden. In the early days the pleader might receive reasonable compensation in the form of fees, in fixing which four points were to be considered: the amount of the matter in dispute, the labour of the serjeant, his value as a pleader in respect of his learning, eloquence, and repute, and the usage of the court. All these precepts have persisted in the present Canons of the American Bar Association.

The professional conduct of the English barristers was not, however, eventually prescribed by statute, but by the Benchers in the four great Inns of Court—Lincoln's Inn, Gray's Inn, Inner Temple, and Middle Temple—where the barristers received their legal training, and which regulated both the barrister's call or admission to the bar, and his disbarment for "conduct unbecoming his profession." The "bar" was a barrier of iron or wood at which the pleader stood when addressing the judges. In the early days a barrister was "disbarred" by being thrown physically over the bar by his indignant colleagues.

The serjeants had their own Inn, called the Serjeant's Inn, to which the judges, who had been serjeants, also belonged, and hence addressed the serjeants as "brother." The serjeants sat within the bar and were addressed by one another—even the oldest by the youngest—by their surnames, without any "Mr."

Although we know of a secular school of law at Canterbury between 1130 and 1176, and of another at Oxford in 1220, the real education and training of the barristers was at the Inns of Court in London. It was thorough and intensely practical and conducted by the senior barristers or Benchers. Although consisting in part of lectures by appointed Readers, its principal feature was the moots, which consisted of arguments and discussions in the hall after supper by benchers and barristers on doubtful cases propounded by them.

Many of the lawyer's duties and obligations which are now defined in the Codes and Canons of Professional Ethics are also covered by statutes and court decisions—such as the duty to observe the utmost good faith and fidelity to the client as a trusted fiduciary—not to serve two masters, or to disclose confidential communications—the duty of respect, candor, and fairness to the court, the duty to have no part in fraud or the abuse of legal process, and the like. There are, however, two obligations which are not enjoined by any statute but have been recognized from the earliest times as inherent in the legal profession and which are obviously the result of the conditions under which the profession developed. It is these which it is my primary purpose to discuss this afternoon.

They are the obligation not, directly or indirectly, to advertise for or solicit professional employment, and the duty not to steal one another's clients.

Obviously these obligations originally developed as a result of the peculiar conditions under which the early English lawyers received their legal training and practised their profession.

These lawyers were an intimate fraternity, dining together in the halls of their Inn, joining in the readings with the students and in the moot courts in which difficult legal problems were discussed and debated, and appearing in court, today as colleagues and tomorrow as opponents. As Shakespeare says of them in the Taming of the Shrew; "Do as adversaries do in law; strive mightily, but eat and drink as friends." Also, they were originally almost exclusively men

of substantial origins, often younger sons of England's leading families, who, for the most part were not dependent on their profession for their livelihood. They regarded all forms of trade with contempt and scorn, and the practice of law in the same way as they did a seat in Parliament—primarily a form of public service, in which the gaining of a livelihood was but an incident. Advertising and competition for practice was deemed ignoble and beneath their dignity. Also, the intimate terms on which they lived and worked would have been impossible for men who were continually blowing their professional horns and plotting to steal away one another's clients, and hence looked down on by their professional colleagues.

The amenities of the profession which were thus developed in the Inns of Court were absorbed and brought to this country by the young men who, in the period prior to the Revolution, went to the Inns of Court for their legal training, and on their return became the leaders of the bar in Boston, New York, Philadelphia, Baltimore, and the south. In Virginia and North Carolina they practised without admission to the local courts—as members of the Inn where they had studied.

There were thirty or forty such men who studied at the Inns before 1760, and more than a hundred between 1760 and the end of the Revolution, including forty-seven from South Carolina, twenty-one from Virginia, sixteen from Maryland, eleven from Pennsylvania, five from New York, and a number from other states.

Included among them were Samuel Dulany, Sr. & Jr., Charles Carroll, Sr. & Jr., from Maryland; John Read, Jeremy Gridley, and James Putnam of Massachusetts, David Ogden and Richard Stockton of New Jersey; from Pennsylvania Andrew Hamilton of whom originated the phrase "as smart as a Philadelphia lawyer," Benjamin Chew, Thomas McKean, and Edward Shippen (who afterwards became Chief Justices), John Dickinson, Francis Hopkinson, and George Read; from North Carolina, James Iredell; from South Carolina John Rutledge (who became Chief Justice); from Virginia, Peyton and John Randolph (each of who became Attorney General); Edmund Pendleton and John Blair (who became Chief Justices), Thomas Jefferson, John Tyler, George Mason, Robert Henry Lee, Patrick Henry, and George Wyeth, Chancellor, who was the preceptor of Jefferson, Marshall, Madison and Monroe.

Owing to the influence and competition of such men as these, high standards of education, craftsmanship, and conduct were established in our early courts and were maintained through the period immediately following the Revolution.

During the second third of the nineteenth century, however, there developed a growing hostility against special privileges granted by the government. Professions, and particularly bar associations, were deemed undemocratic and un-American. This manifested itself everywhere in a lowering of the required qualifications of character, education, and training. In a number of states statutes and even constitutional provisions were passed upholding the inherent and "natural" right of every voter of good moral character to practice law. In others the right of admission was assured after a very brief period of study. There was also widespread objection to the organized bar as a "secret trade union" or a privileged class not open equally to all citizens. This still persists in the Abraham Lincoln argument for easy admission. Even bar meetings, which had been primarily social gatherings of the legal fraternity, or to honor a distinguished judge or lawyer, usually recently deceased, were generally discontinued. In the period immediately after the Civil War, the bar reached its lowest ebb.

About 1875 the leaders of the bar, realizing the deplorable condition into which their profession was falling, as well as the imperative necessity of taking a firm stand against the rising tide of commercialism and the growing influence of those who would turn the profession from a "branch of the administration of justice" into a "mere money-getting trade," began the movement for the reestablishment at the bar of standards of character, education, and training, by means of the organization of bar associations all over the country. This movement has grown and prospered until now in every state it is recognized that, both for the protection of the public and for the maintenance of the law as an honorable profession, no one may practice law whose character has not been subjected to examination and found worthy, and who has not passed a prescribed test as to his character, education, and legal training. Also bar associations are now organized in all the states, in the

District of Columbia, and in a great many countries. These are not merely fraternal societies, holding annual dinners and memorial meetings, or trade associations for the material advancement of their members, but organizations devoted to the pursuit of a learned art in the spirit of public service, and exercising over the lawyers throughout the country a potent influence for the maintenance of high standards of professional conduct, similar to that of the Inns of Court in England.

In an article in the *Atlantic Monthly* in January, 1924, Lord Moulton gave expression to a profound thought, applicable to all of us, but particularly to lawyers:

> The real greatness of a nation, its true civilization, is measured by the extent there of Obedience to the Unenforceable.

In 1908 the American Bar Association, which had been organized thirty years before, adopted, after several years consideration and discussion, a Code of Ethics, consisting of twenty-eight Canons, to which have since been added nine additional ones. These Canons have been recognized substantially, by practically all the state and local associations. They embody the lawyers' idea as to his duties and obligations to the public, to his clients, to the court, and to his colleagues at the bar. No. 7 advises the bar and the public that "efforts, direct or indirect, in any way to encroach upon the professional employment of another lawyer, are unworthy of those who should be brothers at the bar." No. 27 proscribes advertising and solicitation of professional employment, direct or indirect.

While one can readily understand how such practices should have been banned by the atmosphere of the Inns of Court, why is it that they have persisted under the different conditions which obtain in this country, particularly in view of the competitive spirit which has characterized our industrial development and been so potent a factor in its success? If competition is indeed the life of trade, if it is not only proper and desirable for business men to advertise their wares but necessary under the Anti-Trust laws for them aggressively to compete for the patronage of their rivals' customers, why should it be improper for lawyers to do likewise?

Several considerations have led to the persistence of these rules of professional conduct and their inclusion in the Canons of Ethics.

In the first place, while American lawyers are by no means as intimate a fraternity as the English barristers with whom these rules originated, still they differ radically from the milk man, the liquor dealer, or the manufacturer of cigarettes in being yesterday an antagonist and today a colleague on the same side of the counsel table. They also differ in being members of a profession. This is not a fancied conceit, but a cherished tradition, the preservation of which is essential to the lawyer's reverence for his calling—as well as to his regard and esteem for his fellows at the bar. This latter consideration is much more potent than is commonly supposed. Although, prompted by material success, some lawyers may profess indifference to the good opinion of their fellows, actually none is thus indifferent, but each craves such recognition, the more strongly the older he grows. A lawyer who advertises, solicits, or steals another's clients is regarded by his brethren at the bar as one with whom it is not pleasant to associate on the terms of cordial intimacy characteristic of the relationship of lawyers to one another.

Furthermore, advertising, solicitation, and encroachment on the practice of others does not tend to benefit either the public or the lawyer in the same way as in the case of the sale of merchandise. While extensive advertising would doubtless increase litigation, this has always been considered as against public policy. Besides, many of the most desirable clients, imbued with high respect both for their lawyer and his calling, would have no use for a lawyer who did not maintain the dignity and standards of his profession and would instinctively resent any attempt by another lawyer to encroach on their relation. Although other contributing reasons have been advanced for the continuance of these principles,[1] the primary reason for their persistence has, I am confident, been the determination of the leaders of the bar to preserve the dignity of the law as an honorable profession.

There are those at the bar who believe and who have stated that the traditions and amenities of the profession which ban advertising and solicitation of employment and the stealing of clients are being undermined by the go-getter

[1] Such as the tendency to stir up litigation, the evil effect on the ignorant of alluring assurances by the solicitors, as well as the temptation and probability that lawyers who advertise and solicit would use improper means to make good their extravagant claims.

methods of business competition, and that as the personal contacts of lawyers become increasingly less intimate than they were when the "customs of the bar" were developed, they will eventually give way and law become merely another business. I am entirely confident that this will not happen, principally through the influence of the bar associations in standing firm for the preservation of the law as a profession, "a branch of the administration of justice and not a mere money-getting trade."

It is significant that, despite the recognition and adoption during recent years of business efficiency, the reluctance of lawyers improperly to advertise themselves has apparently been strengthened. Judge Sharswood's *Professional Ethics*, published in 1854, contains nothing whatever about advertising. The earliest state Code of Ethics (Alabama 1887) specified as proper "newspaper advertisements, tendering lawyers' services to the public," and condemned only "special solicitations of particular individuals to become clients." In Warvelle's *Legal Ethics*, published in 1902, the author laments the fact that "the leaven of commercial influence is actively at work in the legal profession." He approves the insertion of professional cards, announcing special branches of the profession practiced, in magazines and newspapers of high standing and in village newspapers, although he decries the insertion therein of a residence address, as "bad form." Since the amendments of 1937 to the Canons all such advertisement has been condemned. Of solicitation Warvelle in 1902 says that the rigorous rule forbidding it "has long ceased to obtain more than a nominal observance in England, and never seems to have secured a practical recognition in the United States." Boswell reports Dr. Johnson as having said, relative to the solicitation by lawyers in his time, that he would refrain from it, "not because I should think it wrong but because I should disdain it. . . . However, I would not have a lawyer be wanting to himself in using fair means, I would have him to insert a little hint now and then, to prevent his being overlooked." In England today, however, the ethics and etiquette condemning self-advertisement and solicitation is stronger and more uncompromising than ever.

While there doubtless is at present in the United States solicitation by numerous lawyers, there has not, it is believed, been any weakening by the bar associations of its condemnation.

Evidence of the determination on the part of the bar not to relax the injunction against all advertising by lawyers was furnished by a recent experience of the Ethics Committee of the American Bar Association. Two years ago we were asked as to the ethical propriety of a lawyer's having his name in bold face type in the telephone book. We considered that the telephone book was not a place where a client looked to choose a lawyer, but only to find one whom he had already chosen. Accordingly, since the practice was apparently not intended for advertising, but merely for the convenience of the subscribers, we said that it was not in violation of the Canon. Immediately we received protests from a number of local bar associations and their ethics committees who had ruled otherwise, and who urged us to permit no relaxation of the rules concerning advertising. Accordingly, not to be behind any in maintaining professional standards, we reversed our ruling.

Another significant indication of the determination of the bar associations not to subordinate professional standards to modern business practices was the abolishment of the Finder's Fee as applied to lawyers. It had long been the practice of lawyers, in forwarding a case to a correspondent in another jurisdiction, to expect, as do real estate brokers, a division of the fee, even where the forwarder performed no service and assumed no responsibility beyond the bare recommendation of the other lawyer. In 1928 Canon 34 was adopted providing that a division of fees between lawyers may be based only on a division of service or responsibility.

I believe that never during the last hundred years has the bar been in a healthier condition than it is today. There are, of course, and always will be some whose reverence for their profession and respect for its traditions is so low that they regard it as a mere means of making their living; but they are a minority, although conspicuous because of the prominent place of lawyers in the community. The confidence of the public in lawyers has been continually demonstrated by entrusting them, for five hundred years, with a far larger proportion than any other profession or calling of important public posts in England and the United States. The recently conducted Survey of the Legal Profession has demonstrated that by and large lawyers are observing the Canons and are highly re-

garded in the community by all who have real contacts with them. In my capacity as chairman of the Ethics Committee of the American Bar Association—composed of 50,000 lawyers from every state—I receive continuous inquiries—I had recently eleven in one day—stating difficult questions of professional conduct, with which the inquirers are confronted in their practice, asking whether the Association has taken any position with regard to their particular problem and expressing their desire to conform to the standard of conduct which the Association prescribes.

I am entirely confident that, in this year 1953, the bar is more than ever determined to preserve and maintain the legal profession as one "devoted to the pursuit of a learned art in the spirit of public service."

STUDIES OF THE EARTH'S CRUST USING WAVES FROM EXPLOSIONS

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(Read by Dr. Tuve November 9, 1951)

As PART of a program in geophysics broadly relating to the evolution of continental structures measurements have been made over a period of five years on waves in the earth's crust propagated out from large explosions, usually about one ton of high explosives placed under about 50 feet of water.

We have been interested in determining the character of continental structures down to 50 or 70 kilometers depth, or down to relatively uniform materials, under mountains, coasts, and plains, seeking to describe how continents differ from ocean basins, how the root structure beneath mountains differs from those elsewhere, and hoping, perhaps, for clues relating to the evolution of these structures during past geological time. A continent is supposed to be made up, on the average, of lighter rock than that of an ocean basin; hence it "floats" on the heated plastic mantle of the earth with its surface above sea level. By the same token a mountain chain also "floats" like an iceberg, with its head above the rest of the continent but with a low density root projecting farther into the heavier rock materials beneath. It is of interest to observe and describe these deeply hidden structural features in some detail, with a view to understanding how they may have come about.

Gravity observations give one type of information, and they have shown that the surface features over the globe are roughly in hydrostatic equilibrium, floating on a plastic mantle of heavier rock materials, but most of the detailed indications concerning deeply buried structures have been gleaned not from gravity but from seismic observations, especially on the waves from nearby or local earthquakes.

The work is a joint program of two departments of the Carnegie Institution of Washington, namely the Geophysical Laboratory and the Department of Terrestrial Magnetism. The U. S. Navy, especially through the Office of Naval Research, has made it possible by providing nearly 200 large explosions at times and places specified by us. Numerous colleagues in other institutions have also participated. We are particularly indebted to our friends at the California Institute of Technology; the New Mexico Institute of Mining and Technology; the United Geophysical Corporation, Pasadena; the General Geophysical Corporation, Houston; the U. S. Geological Survey; the Department of Mines and Technical Surveys, Ottawa (Canada); Ontario Department of Lands and Forests; Ontario Department of Mines; Columbia University and to many cordial officials of the mining companies of the Mesabi Range in Minnesota, of the Coast Guard, the Bureau of Ordnance, and the Office of Naval Research. We gratefully acknowledge our indebtedness to many colleagues in the Carnegie Institution who have participated in these studies, especially to W. J. Rooney, who was killed in an auto accident on one of our field trips, to Dr. J. W. Greig, and to our former colleague, Dr. R. W. Goranson.

When our program of observations on explosion waves was begun five years ago, the typical description of a continental structure was a "granitic" layer 10 to 20 kilometers thick, underlying the sediments. Beneath this "granitic" layer was pictured a basaltic layer 8 or 10 kilometers thick, probably covering another basaltic layer of similar thickness but greater density, and at the base of this heavy basalt layer, 30 to 50 kilometers below the sediments, an abrupt transition to the very dense and heavy ultra-basic rock of the mantle of the earth. The sections arrived at have differed materially in different regions, the chief difference being in the presence or absence of sediments above the crystalline rocks, in the thickness of the "granitic" layer, in the assignment of one or of two "intermediate" or "basaltic" layers between the bottom of the "granitic" layer and the Mohorovičić discontinuity, and in the thickness of the "intermediate" rocks. These layers of rock were identified by several apparently discrete values of the compressional wave velocity, from 5.6 km/sec for the granitic rocks to 7.2 for the lower basaltic layer, and 8.1 km/sec for the world wide mantle,

which is believed to exist beneath the oceans as well as the continents. The abrupt transition at the junction of the crust and the mantle, called the "Mohorovičić discontinuity," had been inferred from earthquake times prior to our work; its depth was considered to vary from 20 to 50 or 60 kilometers in different places in Europe and America. The more direct method of locating such a discontinuity, using compressional waves reflected from it at the critical angle, which is especially valuable in the case of explosions detonated on the surface, had not been used systematically. Delayed arrivals of the waves in special localities had been interpreted as caused by the roots of large mountain chains; in some cases the granitic layer was pictured as extending downward many kilometers into the basalt, and usually the basaltic layers were thought to make the Mohorovičić discontinuity underneath large mountain masses bulge downward 5 or 10 kilometers into the ultra-basic mantle rock. This mantle of the earth appears, from long-distance earthquake data, to be relatively uniform and structureless over the globe, extending down to the surface of the core at 2,800 kilometers depth, with a transition of some kind near 800 kilometers.

Earthquakes are not easy to locate with precision in time and space, and the graphs showing apparent wave velocities or travel times, especially for local quakes and short distances, are marred by disconcerting fluctuations among the points. Using the precise timing and locations of explosions we had hopes of reducing these irregularities by observations intended to give a detailed picture of the thicknesses and shapes of these several layers of rocks having different wave velocities, especially beneath such structures as new or old mountain chains.

These hopes did not materialize. Instead our precision measurements have demonstrated that there is a real scatter of the travel-time points and it is due to actual differences in average velocity for different locations, even for different directions from the same explosion point. These differences in travel time are apparently unsystematic and are too great to be caused by the passage of the waves through the stratified rocks overlying the crystallines. Indeed, they are present where the stratified rocks are absent and the surface has been formed on the crystalline rocks. This leads to the view that at any depth the crust is not a homogeneous structureless layer but is built of different rocks in which the waves have different velocities and that the velocity of wave travel between two points is a more or less accidental average.

Our observations give no reason to suppose that the crust of the earth above the Mohorovičić discontinuity is actually made up of sharply bounded layers of rock of different specific velocities or different average velocities. They show no sharp discontinuity lying between the Mohorovičić discontinuity and the upper surface of the crystalline rocks. However, they do show some increase in average velocity with depth and this increase appears to be too great to be attributed to the effect of depth alone, but to require a change in the average mineral make-up of the rocks with depth. According to our observations, the crust of the earth above the mantle is not a set of horizontal layers of rock of different specific velocities, but it appears to be full of irregular structures, and the velocity of wave travel between two points is a more or less accidental average, lying, of course, in a certain velocity range appropriate to rocks.

Our first observations with explosions resembled those for local earthquakes. An average velocity of 6.1 km/sec for the first arrival out to 130 kilometers or so was found although earthquake reports showed 5.6 km/sec. Abrupt increases were also found on our records corresponding to waves apparently arriving by way of the deeper layers of basaltic rock, as described by earthquake observers, and beyond 250 or 300 kilometers waves were observed showing a velocity of about 8.1 km/sec for the deepest paths of travel. The difference between the surface velocity of 6.1 and the earthquake value of 5.6 km/sec, which had been used by many observers for twenty years or more in large numbers of earthquake interpretations (but not by some observers, including Professor Leet and his colleagues at Harvard, for example), occupied our efforts for two years, involving measurements in the east, in New Mexico, and finally an extensive program of shots and observations in southern California. It was found that the difference arose from misinterpretations of the earthquake records for certain regions. Use of the value of 6.1 km/sec (which had been used by Professor Leet and others), and the correspondingly different identifications of impulses on earthquake records in California, also eliminated the traditional difficulty that the shear waves of local earthquakes there mysteriously arrived at the wrong time; they arrive at the time appropriate to materials having a compressional wave velocity of 6.1 km/sec, not 5.6.

When these observations were extended to include many shots in one vicinity (Maryland and Virginia) changing the shot points, the receiver locations, and the directions from shot to receivers, discrepancies were found of ⅓ to ⅔ of a second in first arrival travel times of 15 or 20 seconds, owing to real differences in the earth itself. This real scatter of times was also demonstrated in Tennessee, and in Minnesota, California, and Washington. The abrupt increases of amplitude in the records after the first arrival were not different arrivals or "phases," but were largely fortuitous, owing to the interference of superposed wave trains arriving simultaneously at the receivers over different paths. There were a few very strong second arrivals which were not greatly shifted or obliterated by moving the shot or the receiver. These specific large amplitude second arrivals do not show the characteristics of a local interference pattern, but show the characteristics of a fresh arrival of waves from a lower level. We consider that they are reflections from a transition between rocks of different velocities, hence real evidence for a Mohorovičić discontinuity and a measure of its depth. Lastly, it was noted that the interpretation of the earthquake data in terms of horizontal layers of granitic and basaltic rock was hardly in accord with the degree of consistency among the earthquake data themselves; in effect an arbitrarily simple structure of the crust, in terms of layers, seems to have been assumed many years ago and imposed on the data; it can hardly be said to have been derived from or required by the earthquake observations themselves.

A vigorous search was made, nevertheless, for the intermediate basaltic layers which apparently had been indicated by the earthquake results, and for the bottom of the granitic layer, but without success. If layers exist, they are masked by random inhomogeneities. One structural feature was found with some regularity, and this is an abrupt boundary between crustal rocks (velocity about 7 km/sec) and the heavier rock of the mantle (velocity about 8.1 km/sec) at depths of 30 to 50 km. In some places reflected waves from this boundary fail to appear. Under the west coast mountains, both in California and near Puget Sound, the boundary cannot be located by reflections, perhaps because of exaggerated roughness or because there is no abrupt boundary.

Our present view is that the crustal rocks, which extend from the mantle upward, may well be about as irregular and inhomogeneous as the rocks exposed in any large area where the surface has been formed on the rocks of the Basement complex. These crustal rocks have a thickness of

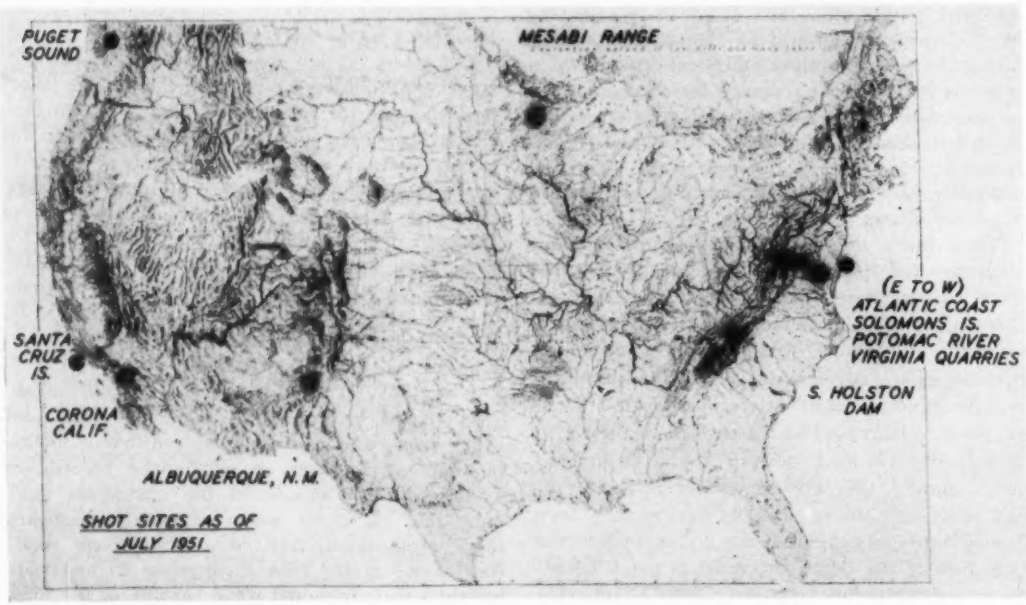


FIG. 1. Distribution of observing regions for various groups of large shots for seismic studies.

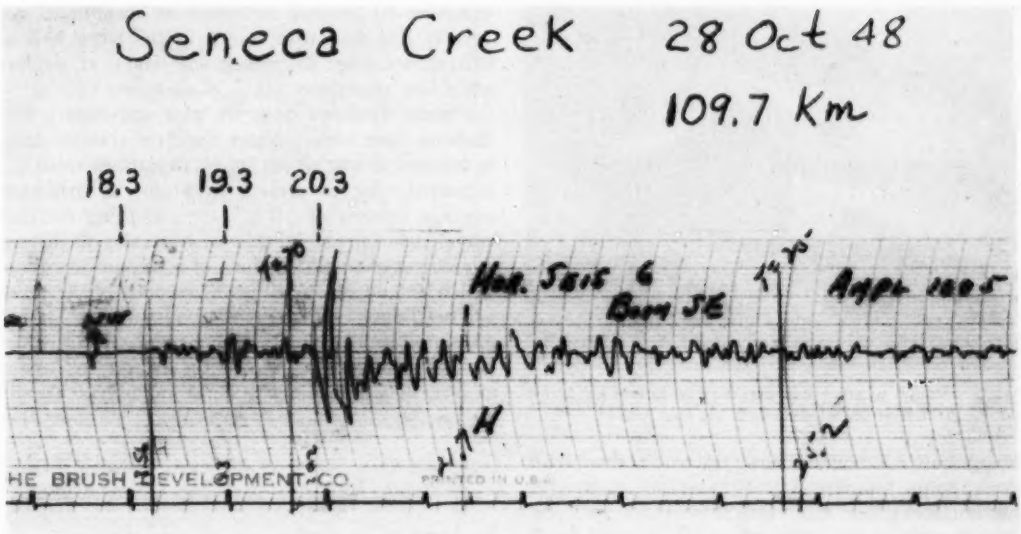


FIG. 2. Recording of a horizontal one-second period velocity seismometer at a distance of 109.7 km from 2400 lbs. of TNT. The upper trace is the pen-and-ink recorded amplifier output with onset of ground motion at about 18.6 seconds after the shot. The lower trace records the one-second chronometer breaks.

30 or 40 kilometers in most areas of this continent, and 45 or 55 kilometers in mountainous regions, and they are more dense on the average (higher velocity) at 20 or 30 kilometers depth than near the surface.

Having described in simple words the general aims of our seismic program and the results obtained, it is appropriate to present a brief technical discussion showing some of the data and original records which have led to these conclusions.

In discussions of such a large scale thing as the crust of the earth one must have primary regard for the limited scope of his own information. We have found it necessary to make large numbers of observations in one locality and expedient to do this near Washington, D. C. The large dots on the U. S. map (fig. 1) indicate the regions where we have had sets of explosions; observing stations have ranged usually out to 300 miles or so, and even as far as 1,400 miles from the shots. The interpretation of the data as showing a crustal structure filled with more or less random inhomogeneities of varied sizes may be due to the particular localities observed, but at least an effort to sample various locations has been made; we expect to examine a few others in coming years. Similarly, we may be misled by special response features of our equipment; no attempt shall be made to discuss this point beyond remarking that we have used displacement, velocity, and acceleration seismometers and have varied the response range over a wide band of frequencies from 1 to 80 cycles. For most of our observations we have used a velocity-type seismometer with response from 4 to 30 cycles per second. Some of the observing positions will be repeated using a displacement seismometer with response from $\frac{1}{8}$ to 4 cycles per second, just to make sure that the emphasis on higher frequencies which is characteristic of a velocity seismometer has not been misleading.

The record for Seneca Creek, Md., using a vertical seismometer at 110 kilometers from a one ton shot, is typical of our field records with portable equipment. Precise timing to 0.02 second is accomplished by calibrating the chronometers at the shot point and at each station against the radio time signals from WWV, which have been most gratefully recorded at all hours and under most varied circumstances all over the United States by our observing parties. Figures 3 and 16 show some of the range of terrain and weather encountered in making about 2,000 separate observations on shot waves. At the distance of Seneca Creek the first arrival was at 18.6 seconds, with typical later arrivals at 19.3 and 20.3 and perhaps others.

During the first two or three years of our in-

FIG. 3. Rough weather was encountered in making about 2000 separate observations on shot waves.

vestigation we usually observed each shot with five to eight separate portable stations, each with one horizontal and one vertical seismometer. By this procedure we acquired a large number of points on travel-time curves for the Piedmont and Appalachian regions in the general vicinity of Maryland, fair curves for various paths in southern California, as well as numerous individual observations elsewhere from New Mexico to Vermont. As remarked above, the times of the first arrivals have a large and real scatter, and most of the later "arrivals" prove to be fortuitous maxima of local interference patterns, not wave fronts suddenly reaching the receiver by way of a distinct path in a deeper layer of rock of characteristic velocity. It should also be indicated that the disturbance of the ground continues for as much as 40 seconds or longer at amplitudes not greatly less than that shown in the latter half of this record (fig. 2), which was taken at moderately low sensitivity.

Figure 4 shows how we plot our data. The distance-time curve, drawn for first arrivals only, is reduced to one which shows deviations from the expected time of arrival based on an arbitrary average velocity of 6.0 km/sec. Plotting reduced travel-time curves in this way enables us to see deviations of small fractions of a second on curves which extend out to 40 or 50 seconds travel time.

The figure showing "Patuxent travel time" gives first arrivals and a few of the more conspicuous later arrivals as shown on a large collection of records made in the general vicinity of Washington, D. C., on Navy shots near Solomons Island, in the Patuxent River. They show approximate agreement with a surface velocity of 6.15 km/sec (6.2 would do as well), and beyond 150 kilometers from the shot the waves arrive early, indicating travel with a higher velocity at deeper levels. In the next figure lines are drawn on the Patuxent data which correspond to a surface velocity of 6.15 km/sec, increasing somewhat with depth, but with an abrupt transition to a velocity of 8.06 km/sec (8.0 would be more appropriate) at a depth of 32 kilometers. It should be noted that the very strong arrivals in the region 80 to 130 kilometers from the shot, recorded one or two seconds after the first impulse and shown as large circles, are interpreted by us as reflections from the upper boundary of the higher velocity mantle rock; the lines are drawn to show what would be expected. The initial delay in the travel-time

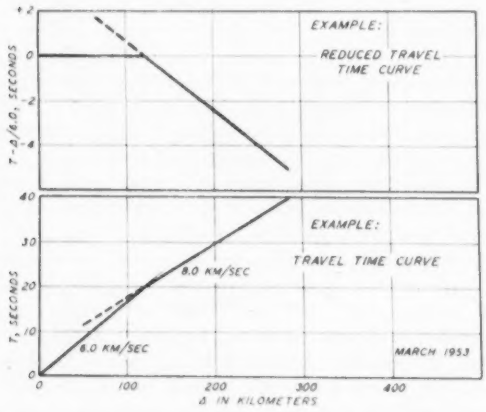


FIG. 4. Travel-time curve; also (at top) the same curve reduced to a standard velocity of 6.0 km/sec.

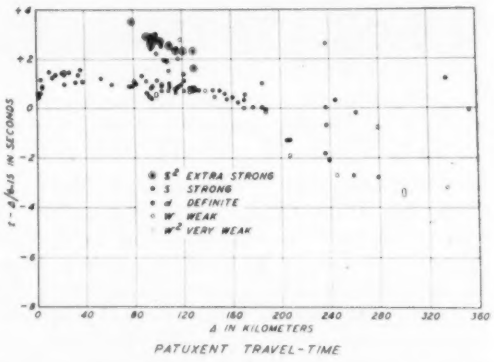


FIG. 5. Actual travel times for about a hundred observing stations in the vicinity of Washington, D. C.

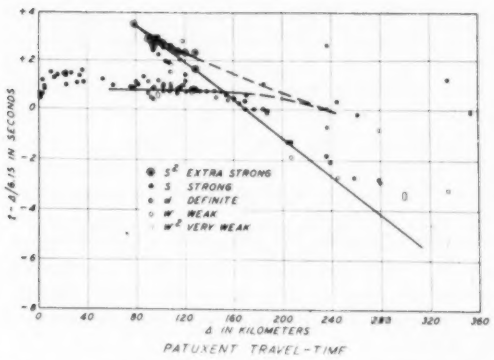


FIG. 6. The observations of figure 5, with curves drawn for an upper layer of 6.15 km/sec and a lower layer of 8 km/sec showing large-amplitude reflections.

curve is due to the low-velocity unconsolidated sediments underlying the Patuxent River to a depth of several thousand feet.

The records of figures 7, 8, 9, 10 show how the observations in various locations and in several regions of the United States fit into a general pattern, with indicated surface velocities of about 6.1 km/sec and with similar variability. Corrections for individual stations do not appreciably decrease the scatter of the points; the time of first arrival repeats precisely for any given pair of shot and receiver points, but it appears to vary in an irregular way, as one moves about. The irregularities appear to be spread over the path of propagation, not just localized at the receiver, analogous, in a sense, to the localized variations throughout the atmosphere which cause the stars to twinkle.

It should be pointed out that hundreds of apparent "second arrivals" are omitted from these

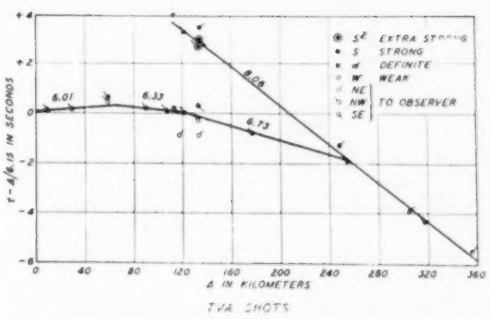


FIG. 8. Observations on several large quarry blasts made by the Tennessee Valley Authority.

plots, because they fail to repeat reliably when either the shot point or the receiving point are shifted a few hundred yards or perhaps a kilometer or two. Shifts like this should not affect wave returns from buried uniform horizontal structures. Even the most vigorous and consistent later arrivals, now interpreted as reflections from the 8.1 km/sec material, do not appear for all directions of the compass from the Patuxent shots, or for all directions at any other shot location. For the Patuxent region they disappear when observing due west and southwest, and this may be due to roughness of the deeply buried boundary, between crustal and mantle rock, for point roughly 50 kilometers west and southwest of Solomons Island. It has not proved feasible to arrange one ton shots under 50 feet of water at all desired locations, but a few observations on quarry shots give support to our interpretation.

The California shots do not show any evidence for the expected sharp transition to a high veloc-

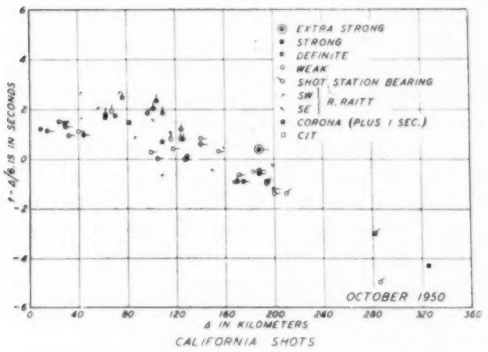


FIG. 7. Observations of first arrivals on seventeen large shots off the coast of southern California.

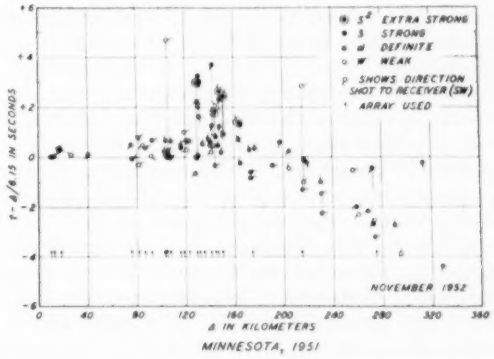


FIG. 9. Observations on about forty large quarry shots in the Mesabi district, Minnesota.

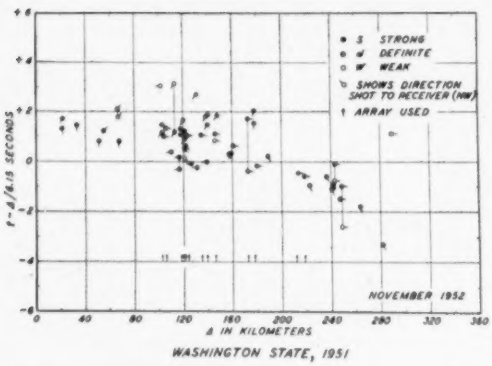


FIG. 10. Observations in the state of Washington, from shots in Puget Sound by the U. S. Coast Guard.

ity (8.1) material. The observations were necessarily limited to a given series of shots, and it is reasonable to consider that the buried boundary in this region is so rough and broken that the reflections from it could not be found, although the observers were placed and shifted primarily in search of the expected reflections. In about sixty observations on seventeen shots no reflections were found.

The variability of the arrival times, even for precisely located and timed shots, shows that caution is highly necessary in the interpretation of local earthquake data for southern California. Since the position and time of a local earthquake requires the data from four independent stations simply to specify the place and time of the earthquake, only the data from additional stations (beyond four) is available for studies of travel time or other items. Earthquakes, in addition, may have actual extension in time and space. Needless to say, we find no evidence in our limited set of records (about 60) which might indicate intermediate layers.

The records from the TVA blasts for mid-Tennessee show reflections from the 8.1 km/sec surface, the Mohorovičić discontinuity, and place it at 45 to 50 kilometers below the surface, while the Patuxent data would place it at 30 to 35 kilometers depth in the coastal region.

In the records for the vicinity of the Mesabi Range in Minnesota we see the reflections from the buried discontinuity, at least for three directions from the various shots. The depth here is about 40 kilometers. In the westerly direction they were not found, again presumably because of roughness.

In the Puget Sound region, as in California, we were unable to find the consistent high intensity "second arrivals" required for identification of a buried reflecting surface. The Mohorovičić discontinuity here does not exist, or, more plausibly, is masked by being rough and broken. The new mountains at the western edge of the continent rise above disturbances in the rocks which extend more than 30 kilometers depth. At the edge of the Olympic peninsula, approaching Cape Flattery, the high velocity rocks (8.1 km/sec) rise rapidly toward the floor of the sea, as shown by observations about 100 kilometers west of the shots in the Sound.

The high velocity rocks at the top of the mantle of the earth (about 8 km/sec) are in evidence wherever we have observed at distances of 300 km or more, but as shown in figure 11, the uniformity of this velocity is in some doubt. A special effort to study these underlying levels was not made, and the idea that the mantle rocks are relatively uniform may be a result of ignorance and very limited measurements. At least there is in many places an abrupt transition at depths of 30 to 55 kilometers to rocks of much higher average velocity.

After several years of observations at separated points we became doubtful of the reality of all previous deductions regarding the crust, including the Mohorovičić discontinuity between crust and mantle, which failed to appear in reflection for some directions from the Patuxent shots and could not be found by us at all in California. We began to suspect that all first arrivals were irregular and unreliable by reason of energy lost in scattering near the surface, and all later arrivals were fortuitous maxima of interference patterns of superposed "reverberation" waves produced by inhomogeneities in the crust of all ranges of size. This would leave nothing reliable in any data from either shots or earthquakes. We are still reluctant to change to an optimistic point of view, but after making many subsequent observations in selected critical locations we have been persuaded by the data to admit that reflections from the discontinuity (or refractions from a transition region of very limited thickness) at the base of the crust (top of mantle) seem undoubtedly real in many locations, and that this discontinuity is deeper under Tennessee and Minnesota than under Virginia and Maryland.

We have sent several expeditions to the field in an endeavor to identify reflections or other

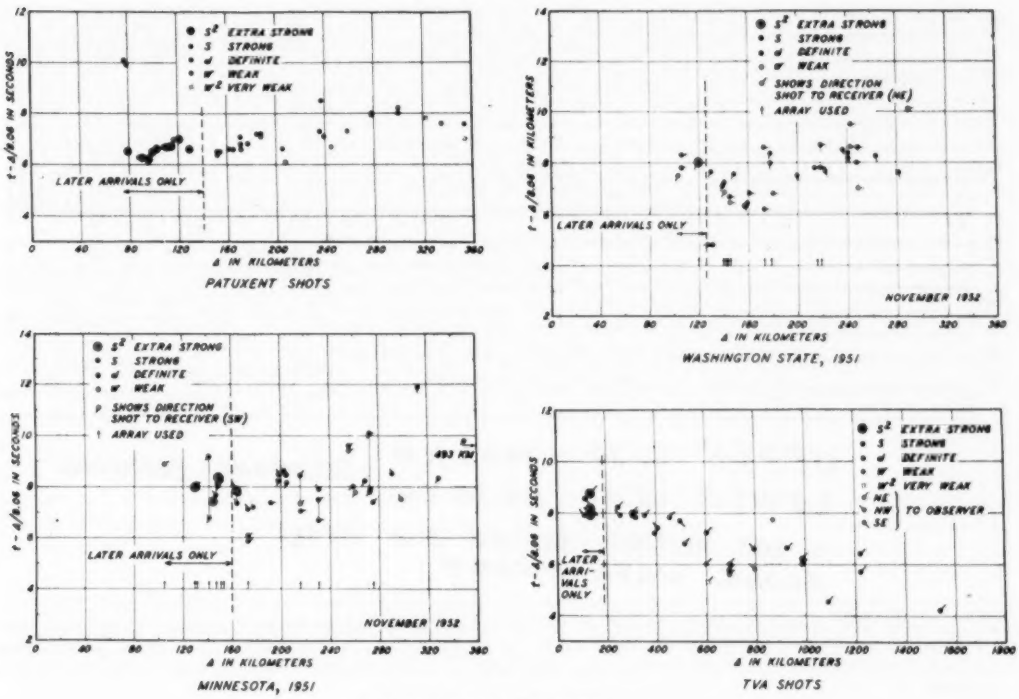


FIG. 11. Wave returns from the high velocity rocks of the mantle (about 8 km/sec) in various regions of the United States.

systematic returns from the boundaries of intermediate layers, such as the bottom of the "granitic" layer which was formerly postulated. These endeavors have been concerned with detailed analyses of the wave pattern over a considerable area, often a kilometer or more on a side, situated at various selected distances from a group of three shot points spaced one or two kilometers apart. These experiments have been carried out near Washington, D.C., in Minnesota, and in the State of Washington.

The analysis has usually been based on observations with a series of instruments placed at intervals of several hundred feet along lines 1,200 to 4,000 feet in extent, usually forming a cross or tee related to the direction of wave travel from the shot. The records show that the received wave pattern differs significantly for seismometers separated by only a few hundred feet. The wavelengths corresponding to our recording frequencies range from 1,000 feet to 5,000 feet, hence the multiple records demonstrate that interference patterns give prominent effects, as of course we knew.

If the shot is moved half a mile or so, many of the features of the pattern received at a fixed group of seismometers 100 kilometers away are changed, as shown in the first half of the Derwood traces in figure 12; the upper and lower sets were for shots 1.3 kilometers apart. Other records show that for fixed shot and receiver locations the traces repeat themselves with great precision. The very strong later arrivals shown (just above the word Solomons) in figure 12 never fail to appear at Derwood and other stations near Washington, in the range 90 to 120 kilometers from any of the Patuxent shots in different locations. These vigorous returns are the ones we have identified as direct reflections from the Mohorovičić discontinuity (or refraction returns from a thin transition region in which the velocity rapidly increases from 7 to 8 km/sec). For a transition layer at a given depth, say 30 kilometers, the

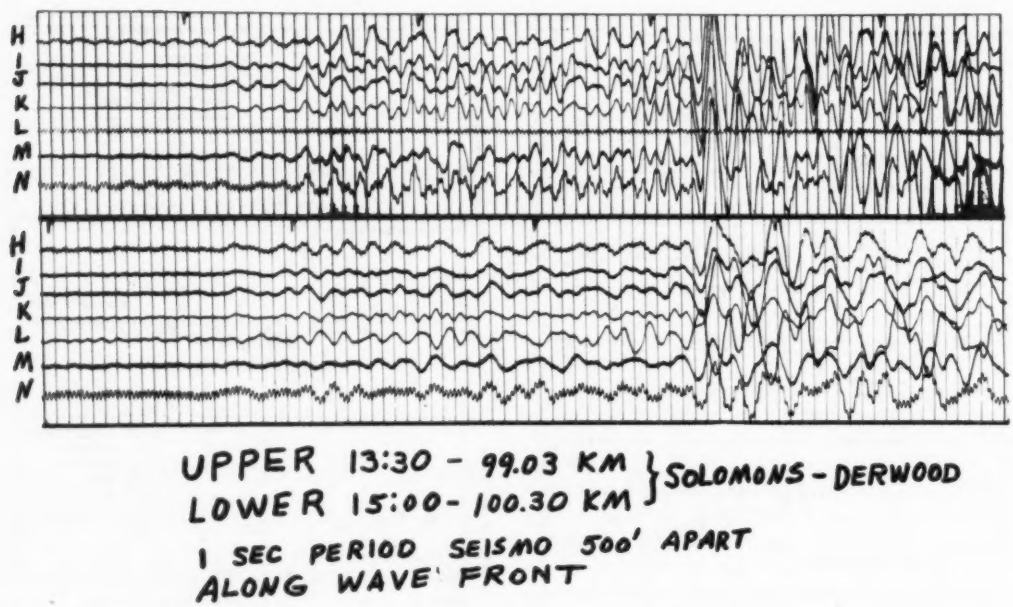


FIG. 12. Early observations showing variations in seismic records at 100 kilometers on 7 instruments stretched over 1 kilometer along the wave front; the lower set is for a second shot 1.3 kilometers farther away. Interference effects obviously make small amplitude features unreliable in single records. The timing-grid lines are 20 per second.

waves will strike it at a shallower angle and be more easily reflected as the distance from the shot increases; for a given velocity contrast at the discontinuity there will be an angle in accordance with Snell's law beyond which the wave does not penetrate the faster layer below, but is totally reflected (except for a minor fraction of the energy which is converted to other modes of vibration). These strong returns occur just beyond the critical angle for reflection at the interface of mantle and crust; the wave energy cannot penetrate downward and a large part of it is returned to the surface just beyond the corresponding critical distance, which is about 90 kilometers near Wash-

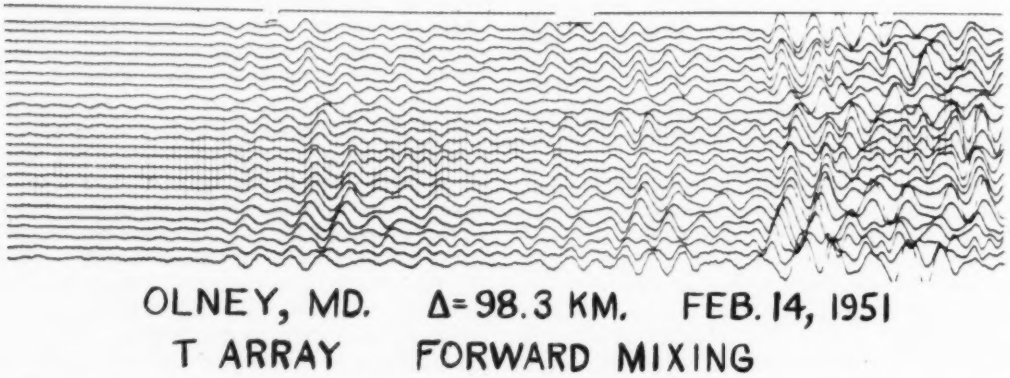


FIG. 13. Typical record at 100 kilometers using 24 seismometers spread over 1 kilometer distances along and perpendicular to the direction of wave travel; one second marks at top.

ington. For distances beyond 125 kilometers these strong reflections fade into the general background of reverberation.

For less velocity contrast between the two types of rock above and below a velocity transition of this kind the critical angle becomes shallower and the critical distance at which the reflections return to the surface becomes greater; it is easy to see that intermediate boundaries between "layers" of low velocity contrast above the Mohorovičić may escape detection by critical reflections if the boundaries are buried 20 or 25 kilometers deep, because the reflections would be exhibited at a distance well beyond 100 kilometers, overlapping other wave returns. It is now realized that, although the methods seem better suited to such a search than decades of earthquake observations would be, nevertheless, for a velocity difference between two deep layers which is very small, say less than 0.2 km/sec, no existing procedure will demonstrate the boundary between them, whether by earthquakes or by explosion methods.

In view of the marked velocity inhomogeneities of structures ranging from large to small dimensions in the crust, as the results of the travel-time, interference, and reverberation studies are interpreted, it seems at least in the regions we have studied that to postulate a crustal structure having horizontal stratifications with small velocity contrast would only be a fictional simplification of the heterogeneous character apparent for the crustal rocks which are exposed. Thus it seems reasonable to think that the crust throughout its volume may carry the scar tissue, so to speak, remaining from a long succession of mountain building epochs. Thus the fact might have to be taken into account that there are lateral as well as vertical variations in the crust. Travel-time observations on seismic waves can give only averages of the actual velocities for locally varying rocks between the points of observation, and that the idea of a uniform horizontal layer of homogeneous rock must be a greatly idealized first approximation was pointed out by Professor Birch in the

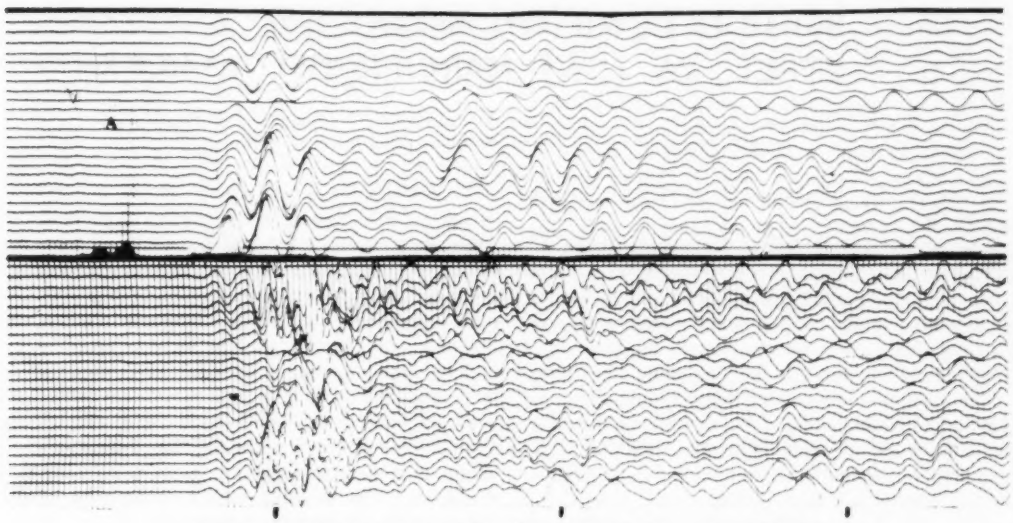


FIG. 14. Records from two sites at the same distance but different orientations from the shot show superposition and interference effects and the futility of identifying "phases" from single records; one second marks at bottom of traces.

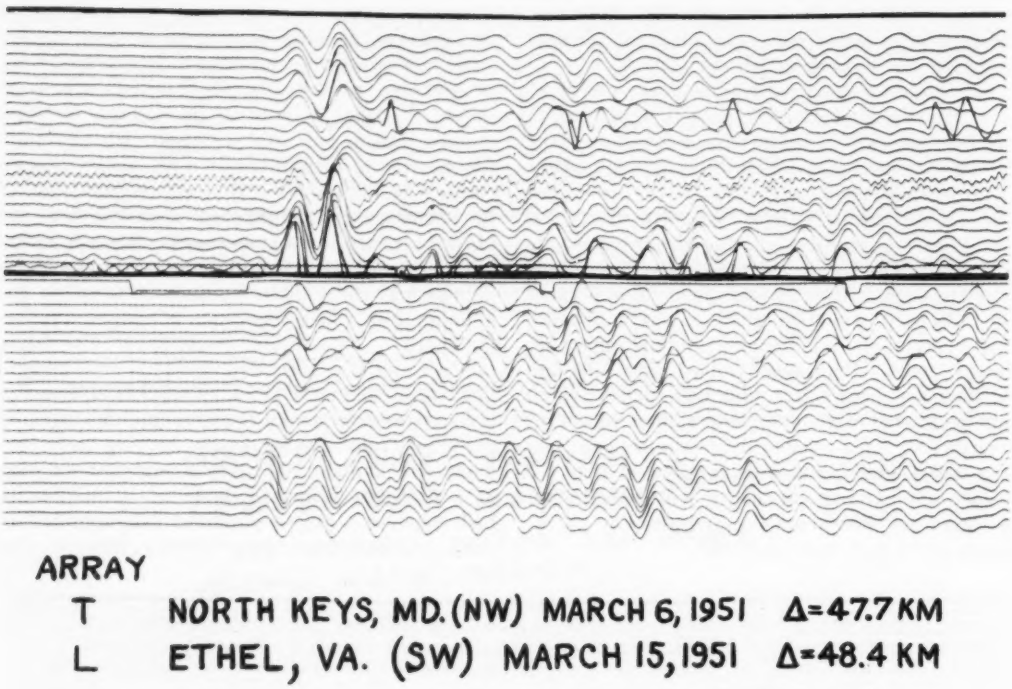


FIG. 15. The search for wave returns from "intermediate layers" is defeated by interference and superposition effects; duration shown is about three seconds.

Bulletin of the Geological Society of America in 1943.

The record from Olney, Maryland, figure 13, will serve as a typical representative of records taken at various distances with the two 24-unit seismometer arrays used during the past two years. Much of this equipment was loaned by the United Geophysical Corporation of Pasadena, California.

The multiple seismometer records from Coles Neck and Aquasco (fig. 14), show how different the records are at similar distances in different directions from the shot. These are two of a number of records we have taken in the search for evidence for intermediate layers. Similarly, for the records from North Keys and Ethel, in which the seismometers were arrayed in the form of a "T" and an "L," respectively, we find no evidence for buried horizontal transitions of an abrupt character; the apparent "phases" shift to new positions when the shot position is shifted only modestly.

It should be remarked that our records and measurements have not all been prepared in final form for detailed publication. It has seemed appropriate and timely, nevertheless, to present this statement of the scope and results of our seismic program, with the reservation that some changes in detail, interpretation or emphasis may occur as we proceed with further studies of our records and the field observations we now have in progress.

The transition in properties and composition from the crust to the mantle remains as the most striking feature of the outermost parts of the earth. One of the purposes of the present investigation was to shed light on the character of this transition. In this connection it may be of interest to recall that the ultra-basic nature of the mantle, which is now widely accepted among students of earth science, was first definitely indicated more than twenty-five years ago by the measurements (and interpolations) at the Geophysical Laboratory.

SUMMARY

The rock structures underlying continents appear to differ from those of the deep oceans; these

differences are significantly related to mountain-building forces and geological evolution. Until very recently evidence from local earthquakes in Europe and America has been interpreted to indicate several horizontal layers of granitic and basaltic rocks below the sediments, each layer 6 to 20 kilometers thick, all of them forming a crust covering the ultra-basic mantle of the earth, which is found at 30 to 60 kilometers depth in different places.

In a cooperative program of several institutions, observations have been made on about 250 shots, each using one ton or more of explosives. The shots were provided by the Office of Naval Research and by various mine and quarry operators. They were observed with high precision at distances of 30 to 300 kilometers and beyond, yielding a total of roughly 2,000 seismic recordings at selected sites. Any group of a few observations in this series is deceptively similar to the earthquake data, with apparent successive arrivals which could be wave groups from different depths. However, using many shots and strings of receiving instruments, and moving the shot points and the receivers, the explosion-wave measurements have shown that most of these successive arrivals are localized patterns impressed on the received patterns by the superposition of waves reaching the receivers, evidently from many inhomogeneous rock structures in the crust. No reflections or other regular patterns are identified from horizontal layers or similar large features at depth, except from the interface of the lower crustal boundary and the high velocity rocks of the mantle, which is found at about 30 kilometers depth along the Atlantic coast, about 45 kilometers under the Tennessee mountains, and about 40 kilometers under the pre-Cambrian shield in Minnesota. The boundary between mantle and crust could not be located, and may be rough and broken, or missing, under the coastal mountains in California or near Puget Sound. It seems reasonable from these observations of the pattern of explosion waves and arrival times at distant receivers to picture the crustal material as exhibiting numerous structures at various depths, as might be inferred by observations over the surface, for example, of the pre-Cambrian shield in Canada. There is good evidence in many places, however, for a relatively sharp boundary, or thin transition layer, between the crustal rocks and the higher velocity rocks of the mantle at depths of 30 to 50 kilometers.

FIG. 16. Much desert terrain was encountered in traveling over 200,000 miles by car and truck for observations on shot waves.

THE SIGNIFICANCE AND TECHNICAL ANALYSIS OF ANCIENT TEXTILES AS HISTORICAL DOCUMENTS

RENÉ BATIGNE AND LOUISA BELLINGER

The Textile Museum, Washington, D. C.

(*Read April 24, 1953*)

ALL works of art and especially those found in archaeological excavations have, as speaking witnesses of the past, an historical value. This is particularly true for ancient textiles which, pertaining to a craft, have a triple significance given by pattern, material, and craft.

For a number of generations art historians have been studying the patterns on textiles but have paid little or no attention to the fabrics themselves. Now it is beginning to be fashionable to pay some attention to the weave of fabrics, and dyes are discussed, but few people have bothered about the spinning of the yarns or the fibers of which they are composed. That study needs at least 20 × magnification and good eyesight. But given those, the analyst can learn much from the fibers and the crafts used to turn them into fabrics. C. J. Lamm and his colleagues in Scandinavia for the composition of fabrics, R. Pfister in Paris for the dyes, are among the first to have opened the way to a more scientific approach to the study of textiles as historical documents through the use of technical analysis. This line of research is still in its infancy but has developed far enough at the Textile Museum to prove a useful tool, corroborating and at times guiding the historical approach. In fact the technical study and handling of textiles should be a separate line of research, parallel to, but distinct from, the study of design. The scope of this field and the knowledge to be gained from its study may be imagined if we realize that certain fibers are indigenous to certain geographic areas; that the physical properties of the fibers decide to some extent whether they require spinning and, if so, how they may best be spun; that the fibers also determine whether, or how, color may best be applied to them, and what types of weave will be practical. When these matters are settled the available looms will limit the type or types of weaving possible, as well as the width of the material. When an inch of plain fabric has been woven the analyst can gather information from the fiber as to its possible geographic location, information from the spinning as to whether the fiber was spun according to its inherent nature and therefore in its native orbit, and information from the weaving as to whether the yarns were being woven in a way usual for some given time and place, while up to this point the art historian has no pattern to study. Consequently the study of the fabric can often tell us something of the history of the design.

To understand this clearly we must first realize that fabrics are fluid not static. Warps and wefts constantly change their relative positions in the web, and even the individual fibers change their positions in a yarn, so basically each fabric is governed by the inherent nature of the individual fibers of which it is made, for the fibers do not lose their particular qualities by being made into yarn. They are not smelted like metals, nor fused like clay, they are merely laid side by side. A long fiber like silk does not even have to be twisted to be woven. Short fibers are spun into a thread, but if the process is reversed and the thread is untwisted, we find disconcertingly at times, that we have once more only separate fibers. If a thread is cut, a microscope will show a straight line of severance; if it is broken the microscope will show few fibers ending in the same plane. What has happened is that few, if any, fibers have broken, they have just pulled apart. For this reason it is easier to break cotton thread than linen as the individual fibers are much shorter. For this reason also, parachutes are made only of silk or nylon, silk being four hundred to one thousand yards long and nylon comparatively endless.

Having begun our approach to analysis with the realization that the individual fibers are not changed in order to make a fabric, it is necessary next to study the specific properties of each fiber in order to understand ancient textiles. Ancient textiles were made mostly of linen, cotton, wool, and silk. Flax grew in Egypt and the linen fiber in its stem is a very fine, round fiber of medium length (it should average 20 inches) and great strength. When it has been wet it will rotate in

an S-direction while drying. All vegetable fibers rotate while drying, some in the S-direction, some in the Z-direction.[1] When linen is spun it makes a smooth, tough thread, and if S-spun according to its preference a linen yarn will tend to tighten with each wetting. In early times, Eastern Hemisphere cotton ranged from three-eighths to one inch in length. It looked like a collapsed tube and normally had twists in it. Because of the twists it did not make smooth thread like linen, as the fibers could not lie close together. When cotton is wet each short fiber turns separately and quite violently. Cotton was Z-spun in India, Persia, and Mesopotamia. It was spun and woven mostly in humid climates. In dry countries short staple cotton could not be kept together long enough to weave. Modern cotton spinning and weaving factories, even when they work on long staple cotton, are humidified. Wool is a larger fiber than either cotton or linen and fairly long. Because of its scales and a certain characteristic called "crimp" the fibers lie even less closely in a thread than cotton fibers. The best thread will stretch and catch at other threads, but being an animal fiber it will not rotate while drying. Silk is a long and very slippery fiber which can be woven with little friction, but once the fabric is off the loom and the tension relaxes the yarns slip out of place easily. Our four fibers, then, are quite different one from the other: cotton, short, uneven, and fragile; linen, medium long, smooth, and tough; wool, medium long, resilient, and catchy; silk, very long and slippery. With such differences in the fibers it is clear why diverse crafts were developed to turn them into fabrics. In places where sheep and goats were raised rather than crops (tradition has it that the first sheep were indigenous to Central Asia and spread from there in all directions), the whole skin was used to make garments even long after spinning of vegetable fibers had begun in other places. The only craft needed in sheep country was sewing. In China silk worms abounded. From four hundred to one thousand yards of silk may be reeled from a single cocoon, so by reeling a number of cocoons at once a very satisfactory long thread, which did not need to be spun, was ready to be woven. Consequently, in that part of the world intricate weaving evolved, but little or no spinning. In India and Egypt, cotton and flax fibers might be made into yarns by spinning. Possibly natives first observed the fibers themselves spinning while drying, and being of an imitative turn of mind, continued the process. At any rate the common spinning in Egypt is S, the preference of flax; in India, Z, where the chief fiber was cotton. After a spinning craft was established in a given region, all fibers spun by craftsmen of that region were apt to be spun the same way, the tradition passing from one generation to the next. For instance, the linen warp of early dynastic fabrics is S-spun in Egypt and so is the wool warp of rugs made at Cairo for the Mamluks and the Ottomans in the fifteenth and sixteenth centuries. However, the crafts of spinning were different in India and Egypt for it takes much more dexterity to make a thread out of fibers one inch long than it does when they are twenty inches long. As a corollary it may be said that cotton spinners made excellent linen thread when that fiber came in their way, but there is no evidence to show that linen spinners could spin cotton at all.

As civilization developed, the people and also the fibers traveled. Wool traveled most, for sheep and goats could walk. The Chinese kept the silk monopoly to themselves for centuries, as silk worms feed on mulberry trees and need a special climate. Flax and cotton, being plants, seem to have kept more or less to their native regions. At some point in their travels shepherds and their sheep met with cotton and flax spinners who tried spinning wool, each after his own craft. Animal fibers have no preference as to direction of spinning so both S and Z twists made good thread. The Z-spun yarns sometimes show superiority, possibly because spinners trained on short cotton could hardly help making top-grade yarn from wool, which was a much longer fiber. Having been introduced to spinning, the shepherds seem to have tried it themselves, possibly at first as a way to use wool shed in the molting season. In earlier times wool in Central Asia had been made into felt, for the scales on the fibers will catch on each other and a fabric may be built up by the interlocking of the fibers without spinning, weaving, or knitting. Felt is still made in Central Asia.

The simple weaves in which our four fibers were first found were influenced by the physical properties of the fibers. Linen and silk were smooth fibers and strong. They were set up close together in the warp, and a lesser number of wefts were woven into them. The warps habitually bent

[1] A yarn has S-twist (spin) if, when held in a vertical position, the spirals conform in direction of slope to the central portion of the letter S and Z-twist if the spirals conform in direction of slope to the central portion of the letter Z.

around the wefts and covered them from view. Cotton was too fragile to be woven if the warp yarns touched each other. Cotton warps were set up with a little space between them so there would be less friction in the weaving. As the warps were spaced it was necessary to beat in an equal number of wefts to make the cloth as firm as possible. This type of cloth showed both warp and weft in a checkerboard effect. Wool was usually tapestry woven, a cloth weaving process in which the warps are spaced and held taut, and the weft which is put in slack, bends around the warps covering them entirely. As the weft is put in slack it can be run behind only a few warps at a time if the slack is to be distributed evenly. Therefore it is this weave which seems to have been responsible for the beginning of pattern weaving since it is relatively simple to change the color at will when the weft does not go from selvage to selvage in a single motion. Until the wool weavers developed this craft, patterns had been applied to finished goods, in Egypt by painting on the linen cloth with a pigment which covered the fabric but did not color it, in the East by reserve dying or printing. These techniques like spinning and weaving were also dependent on the fibers. It was hard to dye linen for the fiber is long and not very absorbent (for this reason it makes excellent dish towels), whereas cotton is short and absorbs dye readily (and makes poor dish towels for they get soaked quickly). Silk could be dyed but did not hold its color as well as wool which dyed easily and held the dye because of all the small scales on the surface.

Silk was a fine and slippery fiber. In the Far East silk thread was used to embroider patterns on silk fabric. When silk thread was imported into cotton country it was used to embroider cotton fabrics also. However, in the Near East where wool was the native animal fiber, wool tapestry patterns were made on wool warp during the weaving of the fabric instead of on the finished goods. When wool was used by linen weavers they also worked wool tapestry areas in their linen webs. So when silk came to Egypt there were no embroidery traditions for the linen weavers to follow. They developed stitches, suggested by their tapestry craft, which were suited to linen fabric. The Egyptian stitches, therefore, were different from those used in Asia, and again the differences were caused by the craft traditions stemming from the physical properties of the native fibers.

For instance, the chief stitches used by the Eastern Abbasids, whose capital was at Baghdad were chain stitch (fig. 1), blanket stitch (fig. 2), and coiled double running stitch (fig. 3). Chain stitch was used all over Asia. It seems to have been indigenous wherever the material to be embroidered is either loosely woven, slippery, or fragile. If we consider the working of a chain stitch, we see that it is eminently fitted to keep the yarns of the ground fabric from shifting their positions and also to give an equal pull on each side of the material. For the thread travels the same course on the front and on the back, and the links of the chain on the front equalize any pull there may be between one stitch and the next. Blanket stitch, which was used for fine work, acts in much the same way. Though the thread is carried from stitch to stitch on the face of the material the crossing of one stitch by the next equalizes the strain. Coiled double-running stitch is equal on face and back, the second course retracing the route of the first exactly, coming to the face where the first course went to the back and *vice versa*. All these stitches, developed in Asia over many centuries, were designed to work patterns with as little disturbance as possible to the ground fabrics and therefore are related directly to the needs of cotton fibers which were fragile and silk fibers which were slippery.

In Egypt the necessities of the fibers were different as were the traditional crafts. In Graeco-Roman times colored tapestry patterns were made with wool in fabrics with linen warp. Wool was a comparatively fat fiber and the Egyptian tradition of setting their warps close together made embroidery difficult. It was much easier to work a wool pattern on the bare linen warps than on finished goods. Even then a number of cloth warps were usually grouped together to make one tapestry warp, in order to keep the wool pattern from bulging out of the goods. Consequently, when silk first came to Egypt the linen weavers had no embroidery traditions in which to use it. As it was a fine fiber and did not spread out like wool, it took time before they learned to use it for a tapestry weft. It is interesting to find that the embroidery stitches are what might be expected when a new technique is suddenly called for as it was by the Abbasid Caliphs in Baghdad who wanted linen fabrics from Egypt with silk tiraz[2] worked in them. Since the linen fibers were tough, pull on the material did not have to be taken into

[2] The word "tiraz" is used to mean both an historical inscription worked into a textile and also the factory in which the work is done.

FIG. 1. Chain stitch.

FIG. 2. Blanket stitch.

FIG. 3. Coiled double-running stitch.

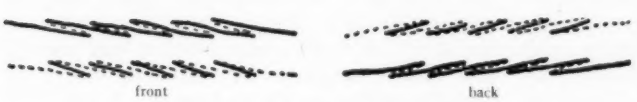


FIG. 4. *Top*, stem stitch; *bottom*, back stitch.

FIG. 5. Flat stitch.

FIG. 6. Couching.

consideration. Linen was not particularly slippery, so it was not necessary to try to keep the fibers in place. The first stitch used was couching (fig. 6). The embroidery thread was brought through the material, laid in the desired place on the face of the material, and carried through to the back again. From the back, couching stitches were taken over the thread on the face to keep it in place. As the thread on the face and on the back coincided only occasionally and took quite different courses between times, this method would work on linen which was strong and stable but was not suited to cotton or silk where the pull on the two sides of the material had to be uniform. Stem stitch (fig. 4, top) and back stitch (fig. 4, bottom) were also used, the one being the reverse of the other. Both stitches tend to pull some ground threads together separating them from their neighbors. Because the traditions behind the craftsmen were weaving traditions, stem stitch and back stitch were worked only in horizontal rows; that is, they were worked on the warps but never went over a weft. Flat stitch (fig. 5) did progress vertically, but again it was a weaver's stitch. The diagonal part of the stitch was on the face of the material and the passage behind the warps was always in a horizontal row. It was the same stitch that tapestry weavers had used with a free bobbin to make vertical demarcations. One other habit of work is used in connection with all Egyptian stitches; they were worked on counted threads. A base line was marked by the pulling of one weft and the letters of a tiraz were worked from that point—so many threads for the width of the letter, so many for its height. This one tradition made it impossible to use chain stitch or blanket stitch. Threads in the ground cannot be counted with a chain or blanket stitch in the way. Coiled double-running stitch also went against the tapestry tradition, in fact against all weaving tradition, of having each weft lie next to the preceding one. In weaving one weft cannot encircle another. Of course, on rare occasions one finds the craftsmen of one region copying the stitches of another region but they usually do it in some way that is apparent if the basic philosophy of the craft is known. For instance, Egyptians sometimes used chain stitch for the tail of a letter, but they did not use it in any part of the letter where the shape was significant. Asians used flat stitch when imitating an Egyptian tiraz but as they had no tapestry tradition behind them, they were apt to turn it sideways and work it over the wefts instead of the warps—a method which would never occur to a tapestry weaver.

From this discussion the fact has emerged that the techniques of spinning, weaving, and even embroidery were originally developed because of the innate properties of the fibers concerned. Spinning was concerned only with the fiber. Weaving and embroidery were concerned also with the pattern, if any. Before weaving can start some fiber must be spun into a yarn which, when set up on the loom, forms the warp. Therefore our basic technical classification of fabrics at the Textile Museum is by warp material. To catalogue a textile as having a single linen warp is comparable to saying that a tune was played on a piano. The same pattern could be woven on a wool warp or a silk one and if this were done the differences in

the result would be similar to the differences in effect of playing the tune on an organ or harpsichord. The basic process is the same but the particular manner of handling the fiber or instrument is different.

A factual approach has always interested Mr. George Hewitt Myers, President of the Textile Museum, so with his advice and encouragement the staff has established a set of working methods for making a systematic technical analysis on which to base their technical classifications. Statistics under three main headings: fiber, spinning, and weave, or in the case of rugs: fiber, spinning, and knot, form a basic analysis now given as part of the Museum registration. After the analysis has been made the fabrics are catalogued historically by pattern and technically by warp material. When a specific group is being studied as a unit, a detailed analysis is undertaken including thread counts and other sub-heads applicable to the particular project. Chart systems have been devised in order to obtain uniformity and clarity of presentation.

Thus far we have given you a synopsis of years of research. Isolated pieces of information in this technical field were baffling until the Textile Museum staff had tabulated enough information on these matters to explain logically phenomena encountered in many phases of work with fabrics which in the twentieth century seem merely to have been ascribed, in the pet phrase of an eminent mining engineer, to "the cussedness of inanimate objects," since the twentieth century has forgotten the beginning of the crafts now so largely mechanized.

For instance, a painter has two linen canvases; one with yarns S-spun, the other Z-spun. He mounts them tight on their respective stretchers and applies a priming coat of paint to each. The S-spun one dries taut and is ready for use; the Z-spun one loosens and sags. The painter and the manufacturer of the canvases had not rediscovered a basic characteristic of vegetable fibers which in the dim past was probably responsible for the beginning of the crafts of spinning. Yet, if the painter were also a gardener, he would know that each type of climbing plant has its own preference as to the direction it will take going up a pole. If he has ever tied a vine firmly in the reverse direction he probably has found the next morning that the new growth was, once more, going its own way. Vegetable fibers even when spun into yarn have the same determination as the parent plant, but as they are somewhat thwarted by being bunched together into yarns which are then beaten together into fabric, this "cussedness" only shows plainly when the fabric has been wet and is drying. When a vegetable fiber is drying it will turn according to its preference. So in the case of the linen canvas with yarns S-spun, the spin will be strengthened and the canvas will be firm; in the canvas with yarns Z-spun against the desire of the linen fiber, the yarns will loosen and the canvas will sag. It would have been useful for the painter if he had known which way a given fiber should be spun to produce a taut fabric. It is indispensable for the staff at the Textile Museum to know how each fiber should be spun before trying to clean an ancient fabric, for if the yarns are spun properly for the fiber they will strengthen with a wetting; if improperly they unwind and the fabric might disintegrate. This characteristic, though not entirely understood, is often taken into account by people who, before cutting material for any given purpose, "shrink" it, that is, wet it thoroughly and let it dry.

Though technical analysis is a major function in itself at the Textile Museum, it serves merely as the starting point for a second line of work which might be called "Archaeological Laundering." This field covers the proper cleaning, blocking, and mounting of textiles. Mr. Myers says he does not like street dirt on textiles any more than on a person's face. Therefore, the staff are investigating the best methods to use in removing dirt without harm to the fabrics. Methods vary according to the fibers, spinning, and weave of the fabrics. The permanence of the dyes used must also be taken into account. For the most part ancient textiles will stand the same scientific treatment as modern ones, provided they are properly supported, so one member of the staff has taken the training course given by the Experimental Laboratories of the Cleaners and Dyers Association. To their knowledge of cleaning agents and methods, she adds experience in handling fragile fabrics. Our cleaning process is designed not only to rid the fabrics of soil but also to lengthen the life of the fabrics by lubricating the fibers, and with this combination of training she is able to bring back much of the original color and resilience of the fabrics, and to hand them on in a pliable state to be properly blocked and mounted, so that they may be shown or studied without coming to harm. The need for proper blocking and mounting is two fold. The first consideration is the preservation of

the textile, the second is to return the pattern to its original state. This second necessity is often overlooked by the scholar who is used to dealing with more rigid media. In sculpture, painting, or carving, the remaining part of a damaged design is still where the artist put it. That is not necessarily so with textiles. The warp, during the weaving of most fabrics is held straight by the loom under a certain amount of tension. The weaver puts in the pattern on these straight warp threads, but when the fabric is finished it is cut from the loom and the tension on the warp relaxes. If the fabric is then stretched diagonally, the warp and weft threads will take new relative positions and the drawing of the pattern may look entirely different. People experienced in working with textiles may be able to make allowance for this distortion, but quite often art historians, accustomed to studying many media, find it hard to realize that textile design is fluid, not static, and that it requires a different approach from sculpture or painting. For this reason it is very important that historic textiles be mounted with their warps straight and their wefts as near as possible where the weaver put them. Some excellent examples of early design have been catalogued as archaic or degenerate just because they were crooked.

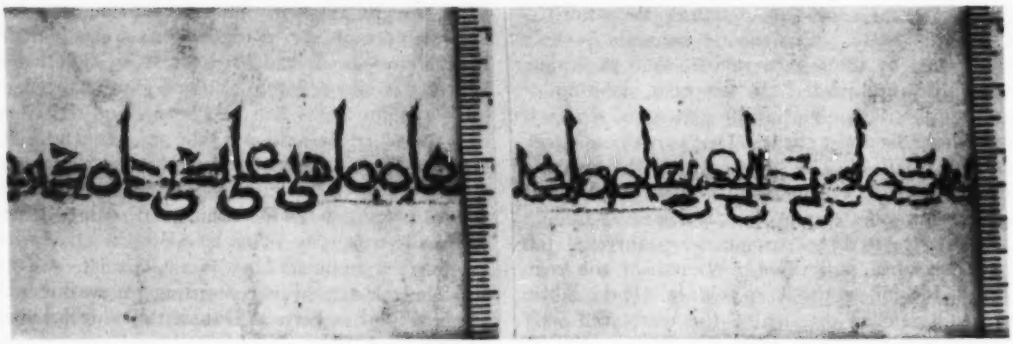
It would be more convenient if the crafts of washing and mounting did not have to go hand in hand with the sciences of analysis and cleaning, but it is necessary for the craftsmen also to be scientists since they must be able to tell what fibers are present in a textile and how best to rid those fibers of the particular type of soil attached to them before any actual work can be done to clean or straighten a fabric. Once the process of cleaning has begun it should, for the good of the textile, be carried through to the end. The textile should be blocked while it is drying and the final mounting should be done while it is still damp. If the mounting job takes so long that the textile dries before it can be finished, the section being worked on at any time must be kept moist with a solution of glycerine and water. We keep an atomizer full of glycerine and distilled water.

In blocking some early textiles it may be impossible to straighten the warps completely. If the fabric lies flat and the warps seem to have been pulled out of line by the weaving, that does not necessarily show lack of skill in mounting. Rather, it is an indication that the textile was woven on a Greek type loom with a beam at the top and weights at the bottom. On this type of loom, the warp threads are held under tension but can still be pulled out of line, as the weights are not stable. Even if the textile had been found in Egypt, that one fact would point to a non-Egyptian weaver, for Egyptian looms had two beams and the warp was therefore held immovable at each end. In this case the student who knew only design and did not know his crafts might easily make a mistake in placing a textile, calling it Coptic when it was really woven by a Greek.

When our technical file was small, isolated pieces of technical data were confusing; now our expanding file has been able to settle some isolated modern problems. It was technical data which allowed the Museum Analyst to suggest the appropriate translation of Esther I, 6 to the Committee for the 1952 Revision of the Bible, thus ending the uncertainty of the 1901 edition which, describing the palace of King Ahasuerus in Susa, read "There were hangings of white cloth, of green, and of blue, fastened with cords of fine linen and purple." A footnote gave "cotton" as an alternate reading for the word translated "green." The Textile Museum files show cotton as the indigenous fiber for that part of Persia; and they also note that indigo was the dye used to produce both blue and green in early times. Good greens will result when indigo is used on yellow wool, but only shades of blue when it is applied to white cotton. The proper translation in this context, therefore, was "cotton" and not "green." The 1952 Revision reads "There were white cotton curtains and blue hangings caught up with cords of fine linen and purple."

Our technical information was put to practical use at least once during the last war. The Director of Navy Research told a member of the staff of dissatisfaction with the uniforms for the men in the South Pacific. When informed that the uniforms were cotton gabardine, the analyst knew immediately that gabardine was such a close weave it would take a long time to dry when it got wet from rain or perspiration, and that all the time the cotton fibers were drying they would be turning and would itch unbearably. She suggested an open weave which would dry faster, made of silk, wool, or nylon fibers which do not spin by themselves.

From these scattered examples it is clear how vast this new field of research can be, and we welcome cooperation from other institutions having textile and rug collections. We are trying to make our methods as simple as possible so that others



face back

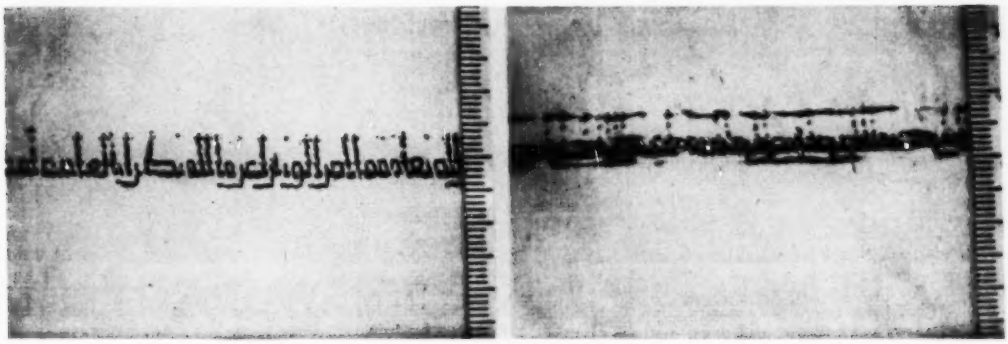
FIG. 7. No. 73.17. Cotton embroidered in the private factory at Baghdad in 310 H. (A.D. 922-923). Chain stitch.[3]

can follow them. They have already been used on the Early Christian and Byzantine collections of some fifteen museums in this country by the Dumbarton Oaks Textile Census, a project started by Mr. and Mrs. Robert Woods Bliss and carried out by Miss Bellinger, who also made a technical file of some two thousand textiles at the Victoria and Albert Museum in London as a collaborative project of the Victoria and Albert Museum, Dumbarton Oaks, and the Textile Museum. Therefore, with our own collections comprising about four thousand items of which three hundred are rugs, over ten thousand analyses are already available. With this information as a background, three papers—Nos. 2, 3, and 6 in the Museum's series of *Workshop Notes*—have been devoted to a discussion of fibers and early techniques in Egypt and the Near East. The textiles from the Yale University Excavations at Dura-Europos were published with technical analyses and the Catalogue Raisonné of the Textile Museum Collections in about twenty volumes will have in each volume an historical treatment by a noted authority in the field and a corroborating analytic study by the staff of the Museum. The *Catalogue of Dated Tiraz Fabrics*, published by the Museum last November as the first volume in this project, has the historical study by Dr. Ernst Kühnel, the technical analysis by Miss Bellinger. As a final example in this paper, we will explain the use of technical analysis, taking as a focal point a particular problem in the Tiraz Catalogue where technical data changed the significance of a textile containing an Historical Arabic Inscription.

In our detailed study of Arabic textiles before the publication of the *Catalogue of Dated Tiraz Fabrics*, Dr. Kühnel's reading of the texts let us arrange the technical information in such a way as to show the workmanship in different regions at various periods, the regions and periods being documented by the inscriptions themselves. In some cases it was even possible to establish the particular characteristics of a given center since forty-four of the texts mentioned the producing factory.

Under the heading *fiber*, we confirmed the fact that all "white" fabrics mentioning Egyptian factories were made of linen; all mentioning Asiatic centers were cotton or mulham (a fabric with silk warp and cotton weft). In other words mulham, cotton, and linen were similar materials but each belonged to a given region. The finished material was exported but not the unspun fiber. Under *spinning* we found that Z-spinning was used in the cottons of the Eastern Caliphate; S-spinning in the Egyptian linen factories. However, during the Abbasid period a little Z-spinning appeared in Egyptian factories. Apparently Asiatic workmen trained to spin cotton were sent to Egypt to insure the type of fabric wanted by the Abbasid rulers. (We mentioned above that cotton countries made fabrics in which warp and weft were equally prominent, whereas in the usual Egyptian fabrics the warp alone was prominent.) They took their method of spinning with them, but did not convert the Egyptians, for when the Fatimids came to power and intercourse with the Eastern Caliphate ended, Z-spinning is no longer found in

[3] The Arabic transcription of figures 7–15 may be found in the *Catalogue of Dated Tiraz Fabrics*, published by the Textile Museum in November 1952.



face back

FIG. 8. No. 7313. Linen embroidered in the public factory at Miṣr in 300 H. (A.D. 913). Couching and back stitch.

Egypt. Fatimid linens are S-spun. Under *weave* we found that the fabrics of this particular group were all plain cloth weave. So, though weave is a basic heading, it was not productive in this project. To these three basic headings two others were added pertaining to this group of textiles: *embroidery stitches*, and *method of working the text*. The *embroidery stitches* (figs. 7 and 8) were different in Egypt from those used in the East. For the reasons explained above Asia used chain stitch predominantly. Egypt used couching, back stitch, stem stitch, and flat stitch. The *method of working the text* was also different in Asia and in Egypt. In Asia the text followed the path of a written script and the embroidery had no rigid relation to the weave of the ground material. In Egypt a thread was drawn for the base line and the inscription was embroidered, from that line, on counted threads making the letters rather angular. With our five headings: fiber, spinning, weave, stitches, and method of work, we had established four good sets of rules which would be useful guides in future analyses.

After these criteria had been set we acquired several more tiraz, one of which (no. 73.661) (fig. 9) is the focal point for this example. Our technical analysis showed the warp and weft to be cotton. The yarns were Z-spun. The inscription was embroidered in chain stitch and the calligraphy was worked like a written script, not from a drawn thread. (A thread had been drawn but the line was not followed exactly, which fact served to accentuate this point.) All the elements were Asiatic. However, Dr. Kühnel's translation of the text read ". . . Blessing from God to the Servant of God Ibrahim, the Imam al-Mutaqqi lillah, in Misr, (Cairo) year 330" (A.D. 941). Here then we had an anomaly. Did the factory at Misr suddently change its habits or did some workman in Asia try to fake an Egyptian tiraz?

Our first move was to examine other tiraz made at Misr. We had fifteen, dating from about A.D. 800 to 971. They all had linen warp and weft. No. 73.372 (fig. 10) was woven in the factory at Misr in A.D. 941/2 (the same year as our problem piece). It conformed to all the general rules for

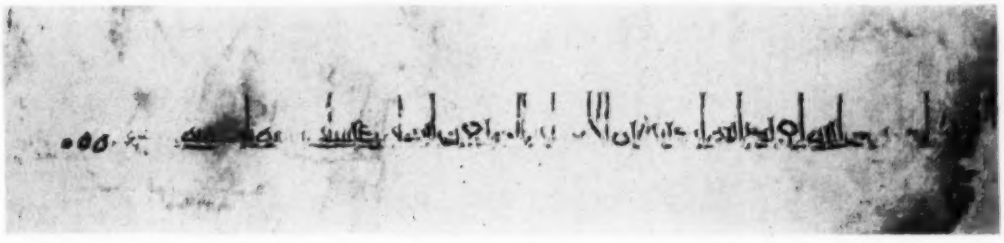


FIG. 9. No. 73.661. ". . . Blessing from God to the Servant of God Ibrāhīm, the Imām al-Mutaqqi lillah, in Miṣr, year 330."

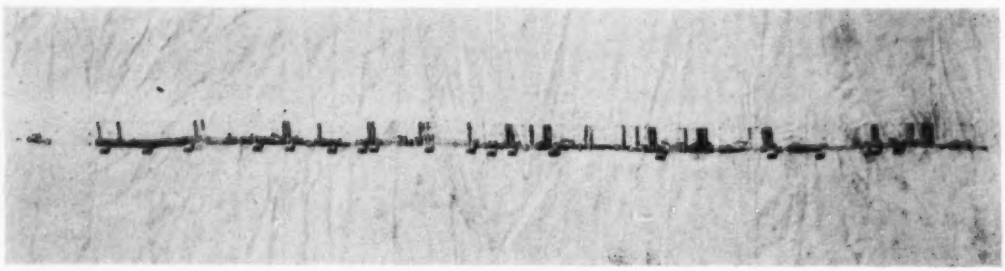


FIG. 10. No. 73.372. "In the name of God, the Merciful, the Compassionate. Blessing from God to the Servant of God Isḥâq, the Imâm al-Mutaqqi lillah, Commander of the Faithful, may God prolong his existence. Ordered by the vazir Sulaymân bin al-Ḥasan, in the factory at Misr. Year 330."

Egypt at that period. The fiber was linen. The yarns were S-spun. The inscription was worked in couching, back and flat stitches, and the individual letters were worked, from a base line, on counted threads. The workmanship also showed the particular characteristics of the Misr factory. The base line was heavier on the back than on the face, and there were several horizontal lines of back stitching which helped to anchor the tails. It was clear that the workmanship of the Misr factory had not changed.

Therefore, no. 73.661 was branded as a fake: the text did not agree with the fabric. But what kind of fake? Was it modern or ancient? Our next move was to compare it with six tiraz mentioning the names of Asiatic factories. It did not quite fit with any of them. Then we compared it with the dated tiraz made in Asia which did not include factory names. It was the same workmanship as 73.635 (fig. 11) made in A.D. 913. The craftsman or factory had kept the same habits for thirty years. So our fake was of known workmanship, attested in the proper century, and was not a modern product.

What then was the place of this textile as a document? From a study of the text we found the protocol irregular since the usual words "Commander of the Faithful" after the name of the caliph were missing, but the date, the caliph, and the producing center were mutually credible. From the technical standpoint the date was probably correct and therefore the caliph also, but the fabric was Asiatic not Egyptian. Mention of Misr in an Asiatic fabric gave the piece a new status which could best be determined by studying the influences shown in Egypt by Asiatic techniques and *vice versa*. As we mentioned before, during the Abbasid period some Z-spinning appeared in Egypt. From A.D. 885 to 961 we had fourteen tiraz with Z-spun yarns. This seemed to show that Asia sent spinners to Egypt to specify the type of fabric to be woven. However, none of these fabrics were embroidered in an Asiatic manner. Therefore, the embroidery trade mark of each factory was apparently acceptable to the Abbasids provided the fabric was of the proper quality. In the middle of this period during the caliphate of al-Muqtadir A.D. 908–932, when Shafi' was Intendant of factories both in Egypt and Asia, we have an Asiatic example of unglazed cotton (no. 73.649) (fig. 12) with the tiraz worked on counted threads in back stitch couching and flat stitch. This tiraz was "made by Muhammad ibn al-Mu'alla" in A.D. 912–913 and we know he was Asiatic because he worked

FIG. 11. No. 73.635. "In the name of God, the Merciful, the Compassionate. Blessing from God to the Servant of God Dja'far, the Imâm al Muqtadir billah, Commander of the Faithful, may God strengthen him. From what has been made in the year 300 (?)."

FIG. 12. No. 73.649. "In the name of God, the Merciful, the Compassionate. . . . God the King, the Manifest Truth, and praise to God, Lord of the Worlds, the Merciful, the Compassionate. Blessing from God and favour from God and felicity from God and protection from God and happiness to the Servant of God Dja'far, the Imām. . . ." Before this text, evidently added later, in red silk, are two words, of which the translation is: "God's is the Kingdom." Beneath the text, also in red silk, the signature: "Made by Muḥammad ibn al-Mu'alla."

his flat stitch over the wefts instead of over the warps. The same intendant evidently brought an Asiatic embroidered to Tinnis, for no. 73.530 (fig. 13) with the end of the text missing was embroidered in chain stitch on linen at Tinnis in A.D. 919 and no. 33.35 from the Dumbarton Oaks Collection (fig. 14) with the end of the text intact came from the same factory a year earlier. The name of the craftsman, Harun (who did not follow the drawn thread exactly), appears at the end of the latter piece and probably appeared on ours originally. Under Shafi', then, who supervised factories in Egypt and Asia, foreign workmen in Egyptian factories or Asians copying Egyptian stitches, signed their work.

Under Djabir, who followed Shafi' in A.D. 934 as Intendant in Egypt but not in Asia, Egyptian factories, while still making the embroidered pieces ordered from Asia also, began weaving inscriptions in their own old tapestry craft using silk for the tapestry weft. By A.D. 945 the whole production was tapestry woven, embroidered pieces being most unusual. Tapestry could not be woven on the fragile cotton warp used in the Eastern Caliphate so the Egyptians may have been showing their growing independence by using their own crafts. That their independence did grow before the conquest is proved by no. 73.632 (fig. 15) made in A.D. 966 while the country was still officially under the government of the Abbasid Caliph al-Muti'. This tiraz, made three years before the conquest, already has the usual shi'ite protocol and bears the name of the future Fatimid ruler al-Mu'izz. In these circumstances, the mention of Misr on an Asiatic fabric may well point to the fact that as early as A.D. 941/2 it was already so difficult to get embroidered tiraz from Egypt that a factory in the East found it worth while to fake them.

In this paper we have tried to show conclusively that ancient textiles to be used as significant historical documents must be studied technically as well as historically. We have shown that because the Textile Museum, as a routine matter, analyzed its acquisitions technically, this particular fabric was spotted immediately as being out of

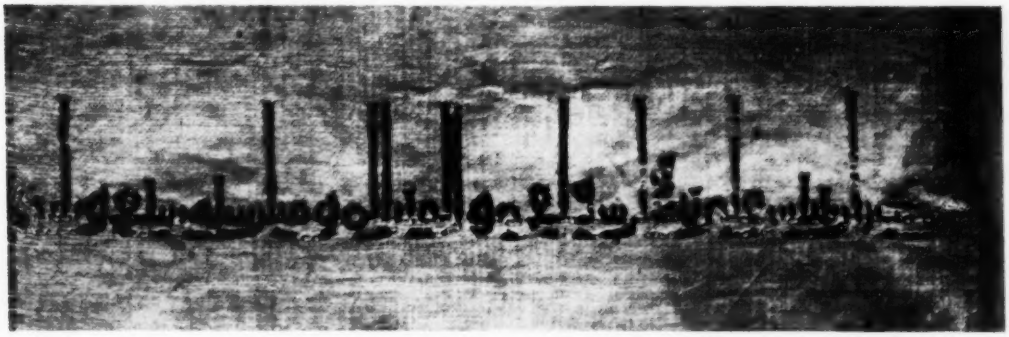


FIG. 13. No. 73.530. ". . . in the factory at Tinnis, under the direction of Shafi', client of the Commander of the Faithful. Year 307."

FIG. 14. Dumbarton Oaks No. 33.35. "(al-Muktadi)r billah, Commander of the Faithful, may Allah strengthen him. What has been ordered by the vazir Hamid ibn al-'Abbas to be made in the factory at Tinnis, under the direction of Shafi', client of the Commander of the Faithful, year 309. Harun."

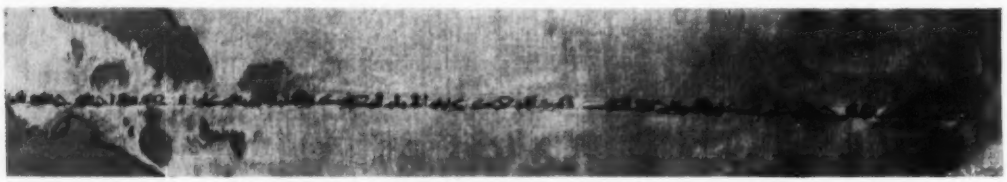


FIG. 15. No. 73.632. "In the name of the Unique God, the Merciful, the Compassionate. Assistance from God and speedy victory to the Servant of God and His friend Ma'add Abu Tamīn, the Imām al-Mu'izz li-dīn-allah, Commander of the Faithful, God's benedictions upon him. Year 355."

keeping with its text. (We can show the same results in other catagories, where a fabric does not fit with its pattern. For instance, a well-known Persian pattern of the ninth century turned up at the Museum on a mercerized cotton warp. The pattern was exact and had been passed by an art historian, but in this particular version the warp was one thousand years younger than the pattern woven upon it, since cotton was not mercerized commercially until the 1890's.) We have shown that when analysis found a dilemma, the fund of technical data at our disposal, gathered from a careful analysis of each fabric, allowed us not only to place this fabric in its proper workmanship group, but also to point out its probable role in the history of trade relations, as a significant document in the chain of events culminating in the decline of Asiatic and the upswing of Egyptian fashion in the Islamic World. For the data which allowed us to orient this textile gave us a graphic insight into the ebb and flow of people and cargoes which travelled on the trade routes connecting Marw, Baghdad, Bishapur, and the Egyptian cities of the Delta. From our own collection we have caught a glimpse of the type of craftsmen who came to Egypt from the East, when they came, and what they did there. We know some types of fabrics made in Egypt to be sent to Baghdad, and the periods at which this trade grew or declined. Comparable data from more fabrics, together with literary sources, might well let us reconstruct a comprehensive history of this trade.

We submit, therefore, that technical analysis of textiles, though a comparatively new field, is a practical one, and should be expanded into a productive discipline, so that ancient textiles may be used with assurance, as significant historical documents.

THE DEFECTS IN CRYSTALS

C. G. SUITS

Vice-President and Director of Research, General Electric Company

(*Read April 23, 1953*)

THE origins of metallurgy lie in antiquity. One of the early handbooks of metals was published in the year 1556 by Agricola, who described how to smelt the ores of tin, lead, iron, copper, antimony, silver, gold and mercury, and how to make crude alloys from them. The title page of Agricola's *De Re Metallica* is shown as figure 1. Agricola's smelter for cinnabar, an ore of mercury, is shown as figure 2. He leaves little to the imagination. *B* is a door, while *C* is a little window. The text explains the function of the trees within the smelter. As the ore is roasted the mercury vapor rises and collects on the leaves as a silvery dew. When the chamber cools the leaves are shaken and the droplets fall and are collected. Apparently the efficiency of this condenser is not exceptional, for Agricola notes that one may observe the very sweet odor of mercury on the leeward side of the smelter. In this event, he advises, go to the windward side, for people who smell too much mercury lose their teeth. His precaution was well taken, for it is now known that one part per million of mercury vapor in the atmosphere is quite poisonous. In spite of occupational hazards then, and now, the results of successful metal working have been so important that people have been willing to go to great pains to achieve them, for metals and alloys pace material human progress. The properties of metallic materials impart the ultimate limit to the power of a steam turbine, the pressure of the boiler, the electrical output of the generator, the efficiency of the transformer, the thrust of the jet and rocket engines, the speed of the airplane,

GEORGII AGRICOLAE
DE RE METALLICA LIBRI XII▸ QVI▸
bus Officia, Inſtrumenta, Machinæ, ac omnia denique ad Metalli▸
cam ſpectantia, non modo luculentiſſimè deſcribuntur, ſed & per
effigies, ſuis locis inſertas, adiunctis Latinis, Germanicisq; appel▸
lationibus ita ob oculos ponuntur, ut clarius tradi non poſſint.

EIVSDEM

DE ANIMANTIBVS SVBTERRANEIS Liber, ab Autore re▸
cognitus: cum Indicibus diuerſis, quicquid in opere tractatum eſt,
pulchrè demonſtrantibus.

BASILEAE M▸ D▸ LVI▸

Cum Priuilegio Imperatoris in annos v.
& Galliarum Regis ad Sexennium.

FIG. 1. Title page of Agricola's *De Re Metallica*.

A—ENCLOSED CHAMBER. B—DOOR. C—LITTLE WINDOWS. D—MOUTHS THROUGH THE WALLS. E—FURNACE IN THE ENCLOSED CHAMBER. F—POTS.

FIG. 2. Mercury smelter from Agricola's *De Re Metallica*.

and to a host of machines upon whose endurance, power, and capabilities modern mechanical civilization relies.

The working of metals had become an important and highly developed art before the advent of modern physical science. It has only been in relatively recent times that some of the important phenomena of practical metallurgy have been explained in terms of fundamental concepts based on atoms and molecules. The primary progress in this direction has come from an application of new knowledge of the physics of the solid state of matter. It is too early to say that efforts in developing the basic understanding of fracture, deformation, diffusion, grain growth, and similar phenomena have been reflected in great advances in metal and alloy technology, but there is a justifiable expectation that this will be so, and substantial progress can be noted. The great antiquity of the metal-working art and the complexity of the modern alloy technology have not made easier the erection of a scientific basis for metallurgy, for a great deal of back-tracking and re-examination of old established principles has been necessary. The task of the metallurgical scientist has been somewhat analogous to that of the contractor who has had to put a new foundation under a very large and very ancient mansion. It would have been easier, and more logical, to erect a sound foundation first, and build the house upon it subsequently, if that choice had been available. The anachronism of the development of an art preceding its science is by no means peculiar to metallurgy, but this field provides an exceptional illustration which is in strong contrast to some of the newer sciences. For example, nuclear science preceded, and still leads by a wide margin, the practical development of nuclear technology.

Because of the historical circumstances surrounding metallurgy, including the fact that its

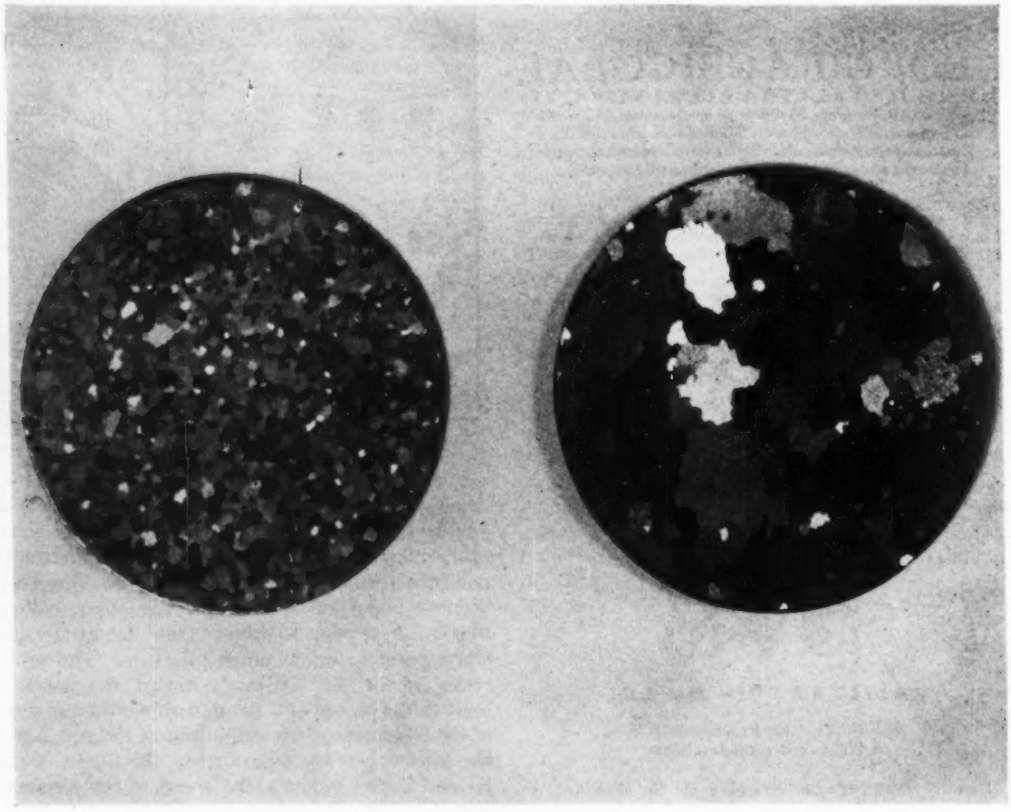


FIG. 3. A photomicrograph of the crystalline structure of two specimens of silicon steel.

practice had attained a great importance before any real science of metals was known, some careful scrutiny has been required to discern just where practice left off, and science began. Some concepts that have been understood intuitively have, upon closer examination, been shown to involve basic fundamentals that were quite unclear, and not at all intuitive. As an example of a simple but very important property of metals, which until very recently has not been explainable in terms of basic concepts, consider tensile strength. Under sufficient tension a wire first stretches elastically like a spring, then it stretches plastically, and finally it breaks. From the pull, expressed in pounds, and from the cross-section of the wire, one can express the tensile strength in pounds per square inch. For lead, at room temperature, this number is approximately 3,000 pounds per square inch; for aluminum it may be 10,000 pounds per square inch; for copper 60,000; for iron, around 100,000, for the strongest alloys, it is about 500,000 pounds per square inch. Now, let's look at this important property in more detail.

From the fact that all metals and alloys are crystalline solids one might seek an understanding of the strength of metals in terms of the properties of crystals. The photograph in figure 3 shows two specimens of silicon steel in which the crystalline character of the metal is readily apparent. The crystals, which may be large or small, fit together and produce an interlocking structure, much like building blocks in a masonry wall. The example may be simplified if one considers single crystals rather than polycrystalline material for, by going to some trouble, it is possible to grow large single grains of a metal. This single-crystal metal, which of course cannot fail inter-granularly, is not stronger, but is much weaker, than the polycrystalline material. As a matter of fact, if the experiment is done with great care and with very pure metal the single-crystal strength is found to be almost zero.

These puzzling results are not easier to understand from a theoretical crystal structure point of view. The tensile strength of this pure single crystal, which has practically no strength, may be calculated by several methods, all of which lead to very high values. The calculation takes into account the known arrangement and spacing of atoms in the crystal lattice, and the known forces between them, and it is not immediately apparent why the calculation should not lead to a correct result. For aluminum as a typical case the comparison between these calculated and measured quantities is shown in the chart of figure 4. Single-crystal aluminum is shown to have a strength of a few hundred pounds, but if the experiment is done carefully it may be much lower. Polycrystalline, but unalloyed, pure aluminum may have a strength of 7,000 pounds per square inch, while the best aluminum alloys show a strength of 75,000 pounds per square inch. Note, however, that the calculated strength of pure single-crystal metal is over a half million pounds per square inch! This failure to account for crystal strength is not confined to the case of single crystals of aluminum; no other single crystals of metal or other substances appear to be substantially better in this respect.

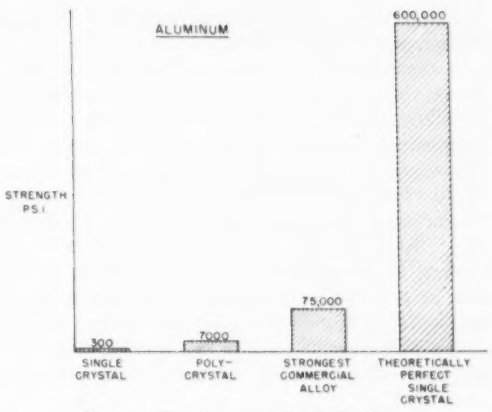


FIG. 4. Tensile strength of aluminum.

The difference between calculated and measured values is particularly disturbing, because concepts of crystalline regularity, as related to structure, phase change, solidification, grain growth, deformation and a host of important phenomena, are fundamental to metallurgical science. In fact, the growth of the modern science of metallurgy can be traced to the origin of the crystal concept, yet, in its application to strength, it fails by a wide margin to account for this basic property. Of course, the failure of the crystal concept to explain strength does not, in any way, detract from its success in explaining quantitatively many other important properties of metals, such as specific heat, thermal expansion, compressibility and energy of formation, all of which depend upon *average* properties of a crystal lattice. However, strength is an exceptionally important property of metal crystals; so much so that serious attention has been directed to a study of this question in recent years.

A clear outline of a quantitative picture of these phenomena may now be seen.

The case cited, and others which could be mentioned, have emphasized the quantitative difficulties of the crystal concept in its application to some mechanical problems in the study of metals and alloys, and have focused attention in recent years on the defects in crystals. Pioneering work on this problem has been done by F. C. Frank and N. F. Mott at the University of Bristol, by A. Cottrell at Birmingham, at the University of Illinois by F. Seitz, at Stuttgart by Kochendorfer, at the Bell Telephone Laboratory by Thornton Reed and at the General Electric Research Laboratory by J. H. Hollomon, J. C. Fisher, and their coworkers.

The crystal defects which explain these mysteries of crystal strength and crystal growth are generally not gross defects of structure, visible to the naked eye, but are more subtle departures from atomic regularity, on an atomic scale. What had been somewhat overlooked these many years is that the crystal lattice is only a model, and practical crystals must contain many defects, one of the simplest of which is an impurity atom. An impurity of one part per million, which is seldom attained with metal crystals, would still permit 10^{16} impurity defects per cubic centimeter. The contribution of one impurity atom per million to specific heat, or to other crystal properties which depend upon an average effect of lattice elements, obviously will not be of much consequence. However, crystal strength is an example of a property that is extremely sensitive to defects. A valuable tool in visualizing such defects has been a special kind of a bubble model of crystals, first employed by Sir Lawrence Bragg. In this ingenious experiment, shown in figure 5, bubbles of uniform size represent atoms in a crystal lattice; their arrangement and behavior simulate to a remarkable degree the perfection and imperfections of actual crystals.

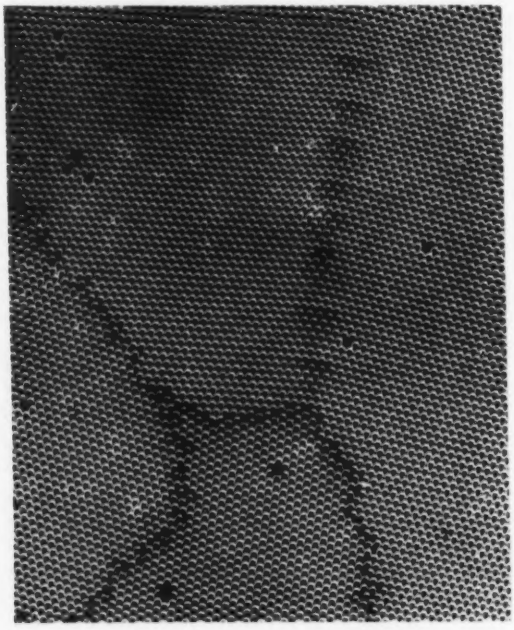


FIG. 5. Bubble model of crystalline defects.

A simple defect may be imparted to an otherwise regular lattice by an impurity atom or by a vacant lattice position; more complex defects involve more or less extensive mismatch between rows and planes of the crystal structure. All such defects and dislocations may produce local lattice strains and distortions of substantial numerical magnitude, which have not been taken into account in calculations based on the strength of perfect crystals, and hence are believed to be accountable for the practical departures from perfect crystal properties. Some calculations have been carried out recently in our laboratory, Dr. John Fisher, to determine in detail the quantitative effect of some typical lattice defects on tensile strength. For the case of alpha brass they show that the magnitudes involved are sufficiently large to account fully for the experimentally observed tensile strength of the actual crystalline material. This recent work has, for the first time, made possible a quantitative understanding of this most important property of crystals. It would be difficult to overestimate the value of this work, for it seems certain that great progress may be expected from a full development of current studies of crystal defects.

The study of the growth of crystals has provided a particularly good opportunity to observe the role of defects and dislocations. When crystals are grown from solution it is observed that the growth rate may vary widely from one crystal to another of the same chemical composition, and this difference can be explained by the relative abundance and character of the dislocations present. A particularly important defect in this case is the lattice displacement known as an edge dislocation. This defect arises from the displacement of many layers of atoms relative to their neighbors,

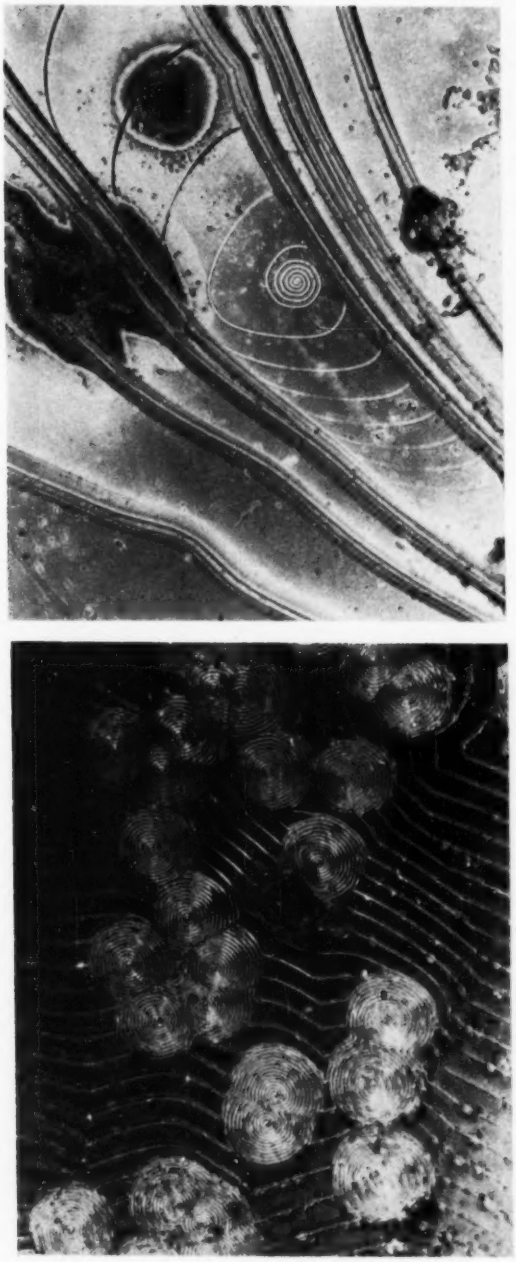


FIG. 6. Photomicrographs of silicon carbide crystal.

on a line perpendicular to a plane of the lattice. Consider the function of this edge dislocation in a crystal experiencing growth from a supersaturated solution. Atoms from the solution migrate to the crystal where they are held by surface forces, but these are particularly weak on an atomically smooth plane of a perfect crystal. In the neighborhood of the edge dislocation, by virtue of the geometry, these forces are very much greater, with the result that practically all of the crystal-growth phenomena take place at these points. The abundance or scarcity of such defects can thus account for a vastly different rate of crystal growth in different crystals of identical chemical composition, the most perfect crystals experiencing the slowest growth.

The relationship between an edge dislocation, and its geometric result—a spiral growth pattern—was first recognized theoretically, by F. C. Frank at the University of Bristol. The first observation of such a spiral, after Frank's prediction, was made by L. J. Griffin on a crystal of beryl from the British Museum. A. J. Forty of Bristol first observed the spiral growth process with cadmium iodide.[1] It will be seen that crystal growth around the edge dislocation leads to a spiral growth pattern, and when this was first realized, many natural and artificial crystalline materials were scrutinized for growth spirals. Such patterns have been found in many substances in addition to cadmium iodide; for example, natural graphite, silicon carbide, gold, silver, magnesium, paraffin and beryl, from which it must be concluded that the edge dislocation is a practically important type of defect that is involved in many crystal phenomena. Some particularly striking growth spirals are found on ordinary commercial silicon carbide crystals of the kind employed for industrial grinding and abrasive purposes. Although pure silicon carbide is a clear transparent substance, the material produced in the industrial electric furnace has a bluish-black sheen due to surface interference colors, by virtue of which the growth spirals are revealed in a striking manner. A photograph of silicon carbide growth spirals taken by Dr. Hubbard Horn of our laboratory is shown as figure 6.

[1] Some pictures of this interesting experiment taken in our laboratory were shown at the meeting, together with motion pictures of the growth of cadmium iodide crystals by means of growth spirals and color photographs of growth spirals on the surface of silicon carbide.

PHOTOGRAPHIC AIDS AND THEIR USES IN PROBLEMS OF AUTHENTICITY IN THE FIELD OF PAINTINGS

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(*Read November 13, 1952*)

THIS paper [1] is not presented as a criticism of scholars and art historians whose methods deal with historical facts derived from the traditional sources of learning. Their contributions in the field of paintings have been vast and they have placed students of history of art in everlasting debt. Present knowledge of the western world's artistic production and personalities would have been impossible without the scholarship of such men. Their learned works are broad in scope and wisdom, and their connoisseurship of the eye has been deeply perceptive. This paper will attempt to deal with the physical aspects of paintings. It is based on the thought that since a work of art is a visual experience it can be analyzed visually and that modern comparative photography can contribute valid information regarding its physical make-up, its condition and its authenticity.

The writer does not imply that the use of photographs in the study of paintings is new. They have played an important role in recording works scattered over the world. For the most part, however, those photographs represented only one aspect of the painting under study. They consisted, in the early days, of prints made from orthochromatic films which, because of their limitations in properly recording color values, were often deceptive. Later the panchromatic film was perfected permitting balanced negatives with respect to color values. The importance of enlarged sections from these negatives was early realized but their usefulness was limited because "grain" began to show in enlargements of great size. The photomacrograph, the value of which was demonstrated by Laurie,[2] is a slight magnification directly on the photographic film. With the use of fine grain developers these magnified images may be enlarged still further with little or no loss through "grain" and the resulting prints become extremely effective in recording conditions frequently overlooked or forgotton in visual examination under a glass.

EXAMINATION BY INFRA-RED, X-RAY, AND ULTRA-VIOLET RAYS

In recent years infra-red photography [3] has been utilized to record subsurface conditions invisible or scarcely visible to the naked eye. Such photographs often reveal repainted areas, modifications made by the artist in developing his theme, and at times even show the basic linear structure or drawing of a painting. The remarkable results obtainable from infra-red photography may be judged by comparing figure 1, which shows a *Flight into Egypt* by Lucas van Gassel (*ca.* 1480–*ca.* 1570) as recorded by panchromatic film, with figures 2 and 3, infra-red photographs [4] of the same picture. In figure 2 the artist's drawing of

[1] The several problems presented here have resulted from studies carried on at the Philadelphia Museum of Art in collaboration with David Rosen, Technical Advisor of the Museum. The writer wishes to acknowledge his debt to Mr. Rosen in the matter of assembling the photographic material used and in reaching conclusions concerning it. Mr. Rosen has also given invaluable assistance in the preparation of this paper.

[2] Laurie, A. P., La microphotographie appliquée à l'étude de la technique de Rembrandt et de son école, *Mouseion* **15**: 5–7, 1931; Laurie, *The brushwork of Rembrandt and his school*, London, Milford, 1932.

[3] Coremans, P., Les rayons infra-rouges, *Brussels Mus. Roy. Bull.* **10**: 87–91, 1938; Keck, Sheldon, A use of infra-red photography in the study of technique, *Tech. Stud.* **9**: 145–152, 1941; Lyon, R. Arcadius, Infra-red radiations aid examination of paintings, *Tech. Stud.* **2**: 203–212, 1934; Marceau, Henri, and David Rosen, Daumier: Draftsman-painter, *Walters Jour.* **3**: 9–41, 1940; Rawling, S. O., *Infra-red photography*, London, Blackie, 1933.

[4] These photographs were made with 5″ × 7″ Eastman infra-red sensitive film, Wratten no. 89 jet black filter, 3200 Kelvin lights, 12″ Dagar lens, f 16 opening. Exposure time was about 16 times the meter reading for the emulsion speed of panchromatic films. The infra-red technique also admits of direct enlargements on the film. It is possible to extend further the legibility of the record by making two prints from the same negative—a short exposure for the dark areas of the print and a longer exposure for the light areas.

FIG. 1. Lucas van Gassel: *Flight into Egypt.* Panchromatic photograph.

FIG. 2. Lucas van Gassel: *Flight into Egypt.* Infra-red photograph.

FIG. 3. Lucas van Gassel: *Flight into Egypt.* Infra-red detail.

the distant mountains and village at the upper right becomes visible; and in figure 3 we see a modification in composition of the middleground—the group of horsemen having been drawn over the earlier indications of a gabled house. In the finished picture this house has been shifted a little higher and to the left of its original position. Modifications of this nature, when they are found repeatedly in the production of a painter, indicate characteristic work habits and serve as guides to recognition of the artist's hand.

The value of the X-ray shadowgraph in the study of paintings has been fully demonstrated by investigators in many countries and an impressive bibliography [5] concerning the technique of making and interpreting X-ray shadowgraphs is available. The technique has many positive advantages and certain limitations.[6] On the positive side it af-

[5] Boissonnas, H., La radiographie et le controle de l'authenticité des tableaux, *Mouseion* **35–36**: 139–143, 1936; Burroughs, Alan, *Art criticism from a laboratory*, ch. 7–8, Boston, Little, Brown, 1938; Burroughs and Robert H. Morris, X-rays in the study of paintings, *Photo Technique*, 36–38, Oct. 1941; Coremans, P. B., *Van Meegeren's faked Vermeers and de Hoogh's*, London, Cassell, 1949; Elliott, William J., The use of the roentgen ray in the scientific examination of paintings, *Amer. Jour. Roent.* **50** (6); 779–790, 1943; Keck, Sheldon, *A technical examination of paintings*, Brooklyn Museum Press, 1941; Keck, The technical examination of paintings, *Brooklyn Jour.*, 71–82, 1942; King, Edward S., Stuart's last portrait of Washington—it's history and technique, *Walters Jour.* **9**: 81–96, 1946; Kurz, Otto, *Fakes—a handbook for collectors and students* 22–27 New Haven, Yale, 1948; Marceau, Henri, and David Rosen, A study in the use of photography in the identification of paintings, *Tech. Stud.* **6**: 75–105, 1937; Pease, Murray, A note on the radiography of paintings, *Met. Mus. Bull.* **4**: 893–898, 1946; Rawlins, F. Ian G., Physical factors in x-ray photographs, *Tech. Stud.* **7**: 73–79, 1938; Rawlins, X-rays in the study of pictures, *Brit. Jour. Radio.* **12**: 239–245, 1939; Rosen, David, and Julius S. Held, A Rubens discovery in Chicago, *Walters Jour.* **13–14**: 77–91, 1950–1951; Weil, Adrienne R., Etude aux rayons x de l'hétérogénéité des précipitations dans un alliage or-argent-cuivre, *Studies in Conservation* **1**: 31–38, 1952; de Wild, A. Martin, *The scientific examination of pictures* ch. 8, London, Bell, 1929; Wilde, Johannes, L'examen des tableaux à l'Institut Holzknecht de Vienne, *Mouseion* **16**: 18–25, 1931; Wolters, Christian, *Die Bedeutung der Gemäldedurchleuchtung mit Röntgenstrahlen für die Kunstgeschichte*, Frankfurt, Prestel, 1938.

[6] Bauer, Victor, and Helmuth Rinnebach, L'examen des peintures aux rayons-x. Son importance et ses limites, *Mouseion* **13–14**: 42–69, 1931.

fords a clear record of the location and extent of pigment losses and indicates facts concerning canvas or panel structure and condition. It records changes made by the artist (fig. 4) in developing his final design, repainting executed by restorers and the presence of hidden paint films which occur when painted supports have been re-used (fig. 5). It cannot be denied that such evidence is of prime importance in the field of restoration and that it may furnish clues to authorship. Indeed, in the celebrated trials of Otto Wacker, the wholesale creator of van Gogh-like pictures, evidence [7] assembled by means of X-ray photography was admitted in both trials and proved invaluable

FIG. 4. Jerome Bosch: *Adoration of the Magi.* X-ray detail.

[7] Wehlte, Kurt, Zum Wacker-Prozess in 2 Instanz, *Kunst-u. Wirtsch.* **13**: 207, 1932; Wehlte, Die maltechnische Röntgenographie im Prozess um die van Gogh-Fälschungen, *Kunst-u. Wirtsch.* **14**: 7–12, 1933. In the first trial six shadowgraphs were presented and thirty-four were used in the second trial. Characteristics of brush strokes, crackle of the paint films and the weave of the canvas were demonstrated. It was found, for instance, that the forger's canvas was of close weave indicating a fine grade of linen whereas authentic paintings by van Gogh were painted on cheaper canvas of coarse weave.

FIG. 5. Gustave Courbet: *Une Dame Espagnole.* X-ray.

in securing conviction. On the negative side we must admit that, unlike the X-ray examination of humans where the basic structure admits of a fairly uniform technique, the unpredictable inner structure of paintings introduces elements of uncertainty as to radiographic technique and thus may lead to a situation where X-ray evidence must be discarded. In other words, X-rays cannot serve as a miraculous and unfailing substitute for knowledge, taste, and the all-important sense of what constitutes a work of art.

While on the subject of radiography, it seems appropriate to mention the mooted question concerning the possible damage to paintings resulting from exposure to X-rays. This question was actively argued in Germany in 1928 and was the subject of articles by proponents of both views. The controversy was brought to focus in 1936 by Albert Weigel's [8] claim that certain pictures in the State Picture Gallery at Kassel had been permanently damaged by exposure to X-rays. Helmuth

[8] Weigel, Albert, Die Geröntgten Rembrantgemälde der Staatlichen Gemäldegalerie Kassel, *Tech. Mitt. Malerei* **52**: 3–6, 1936.

Rinnebach [9] and Kurt Wehlte [10] have convincingly demonstrated that the exposure required for radiographic examination causes no damage to paint films, oils, or varnishes. Although this view has been generally accepted, doubts still find expression among private owners and an occasional dealer. Public institutions interested in assembling factual data have long accepted X-rays as both safe and useful.

Other visual tools include photography under ultra-violet rays [11] and the raking light or critical-angle photograph. The first of these is only occasionally effective in the study of authorship because the protective coating of varnish, present on most paintings, has a fluorescence of its own under ultra-violet rays thus causing a clouding of the paint film. So far, ultra-violet ray examination has yielded its best results in the study of solid objects and as an aid to the restorer after he has removed the varnish from a painting and exposed the paint film. At this point ultra-violet rays will indicate the presence and extent of repainting. The raking or critical-angle photograph, made by using one source of light placed at a sharp angle to the subject, contributes to the study of surface textures of paint films, canvas, or other supports. These details are normally visible to the naked eye but the critical-light photograph can greatly exaggerate and record surface textures for purposes of comparison.

A BURGUNDIAN PANEL

The first problem to be explored by the photographic method concerns a small panel [12] ascribed

FIG. 6. Attributed to Burgundian Artist c. 1450: *Portrait of a Young Lady of Sixteen.* Panchromatic photograph.

to an anonymous Burgundian artist and dated in the neighborhood of 1450. It represents a young lady attired in Burgundian headdress and *décolletage* of about 1440–1450 (fig. 6). She faces to our left and holds a falcon in her right hand. The arms displayed at the upper left denote her royal rank and her age is recorded on the scroll—*Aetatis Suae* XVI. This portrait is related in most startling fashion to a picture (fig. 7) in the Stoclet Collection, Brussels, which has been wrongly identified [13] as the portrait of *Isabeau de Bavière,* wife of Charles VI of France.

Comparison of figures 6 and 7 indicates that, aside from variations in scale and details of costume, the drawing of the features and the drawing and slope of the head and neck in both pictures are identical but in reversed sense. Indeed, these details, when brought to the same scale photographically and super-imposed, match almost line for line. It may be seen that the flesh areas in both pictures exist in terms of their linear struc-

[9] Rinnebach, Helmuth, Zur Frage der Schädigung von Gemälden durch Röntgenstrahlen, *Tech. Mitt. Malerei* **52**: 55–60, 1936.

[10] Wehlte, Kurt, Untersuchungsergebnisse über die Frage von Röntgenschäden an Gemälden und ihre praktische Bedeutung, *Tech. Mitt. Malerei* **52**: 175–178, 1936.

[11] Eibner, A., Les rayons ultra-violets appliqués à l'examen des couleurs et des agglutinants, *Mouseion* **21–22**: 32–68, 1933; Lyon, R. Arcadius, Ultra-violet rays as aids to restorers, *Tech. Stud.* **2**: 153–157, 1934; Rorimer, James J., *Ultraviolet rays and their use in the examination of works of art,* N. Y., Metropolitan Museum of Art, 1931.

[12] Barnes, Albert C., and Violet de Mazia, *French Primitives and their forms,* 82, note 8, 466–471, 469 repr., Merion, Barnes Foundation, 1931; Soria, Martin S., Letter to *Connoisseur* **117**: 128–129, 1946, notes Belle da Costa Greene as first to question authenticity of this panel, calling it the work of the "Spanish Forger;" Valentiner, W. R., *Catalogue of a collection of paintings and some art objects* **3**: 262 repr, no. 760, Philadelphia, 1914. Water color ? on pine, H. 12⅛" × W. 8⅞" including the frame which is attached to the face of the panel.

[13] *Burl. Mag.* **36**: 157, 315, 1920; **37**: 57, 112, 1920.

ture, modeling being reduced to slight hatchings which fail to produce a painterly illusion of third dimension. It is also apparent that the treatment of costumes is linear in character, the decorative motifs of the several materials being drawn in flat patterns of almost equal size and value with little effort to create solid form through perspective and modeling. The latter, where it exists at all (fig. 6), is restricted to shadings along the edges of lines marking folds of the headdress. This method of delineation is precisely that found in figured tapestries of the fifteenth and early sixteenth centuries (fig. 8). In this detail [14] we recognize the same linear method of portraying features, similar shading along structural lines, and the use of flat patterns of decoration with little or no perspective to establish depth in space.

FIG. 7. Presumed *Portrait of Isabeau de Bavière.*

[14] Reinach, Salomon, *La tapisserie gothique*, pl. 81, Paris, Demotte, 1924.

FIG. 8. Brussels, early sixteenth century: *Tapestry.* Detail.

Let us next examine the shadowgraph (fig. 9) of the *Portrait of a Young Lady of Sixteen.* In a panel of this period one would expect the shadowgraph to register some trace of the painted surface. The pigments, either in tempera or in mixed oil and tempera medium, should possess sufficient density to intercept X-rays in some degree and produce shadows consistent with the composition. Our shadowgraph fails to do this but it instead records a heavily crackled condition which must be independent of the painted surface and therefore must exist in the ground itself. Granting for the moment that the paint film is genuine but unusually thin and transparent to X-rays, we must still account for the character of the crackle pattern in the ground. The cleavages are deep and extend to the wooden support. The separations are not uniform in character but are interrupted by numerous discolorations presumably cast by old losses and fillings made with materials of varying densities. These repairs should normally extend to the painted surface and be visible as repainted areas. Examination of the painted surface fails to locate such areas and we must conclude that

FIG. 9. *Portrait of a Young Lady of Sixteen.* X-ray detail.

the ground was damaged and filled before the painting was executed. Since we are dealing with an unusually thin paint film, it is apparent that the crackle visible on the picture itself (fig. 11) was induced by the shrinkage of the ground. If we compare this condition with that existing in figure 10, a portion of a panel by Giovanni di Paolo,[15] we note that the crackle pattern lies in the paint film itself and does not extend beneath it to the denser *gesso* ground. Indeed, in the small damaged spot at the top of the halo the crackle pattern is broken and a relatively smooth bit of the *gesso* is seen below. To illustrate this point further it may be said that the fine crackle in the paint films of old panel pictures resembles the crazing of porcelain surfaces in which the pattern of fine cracks exists in the glaze and does not extend to the support upon which it has been placed.

Photomacrographs (figs. 11 and 12) of our panel also yield conclusive evidence. Figure 11 shows the nature of the brush strokes used in building the flesh tones of the face and neck. These strokes consist of hatchings and scumbles lacking in functional character and serving only to "fill in" the spaces confined within the drawn

[15] Berenson, Bernhard, *Catalogue of a collection of paintings and some art objects* **1**: no. 105, Philadelphia, 1913.

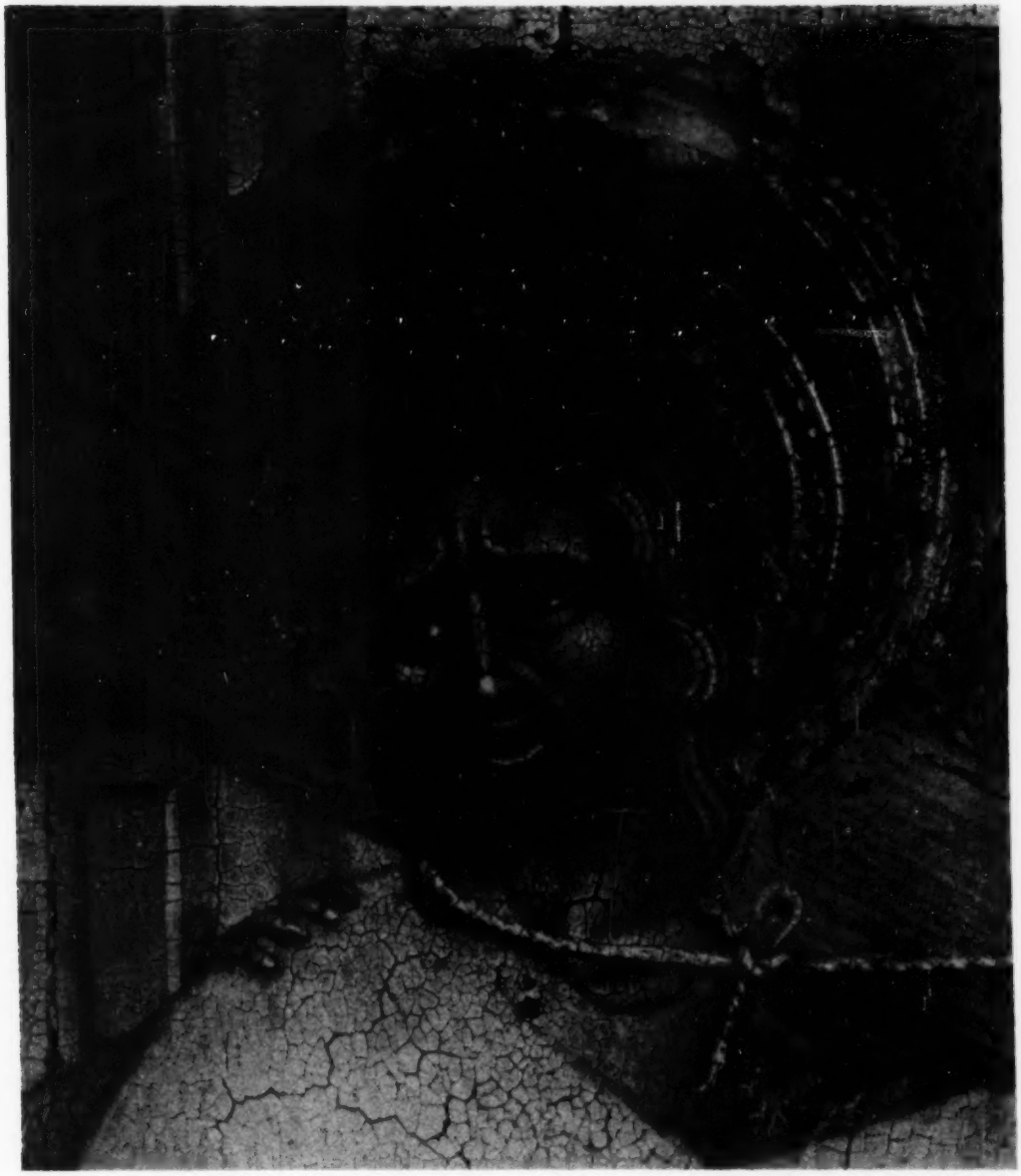

FIG. 10. Giovanni di Paolo: *Christ Carrying the Cross.* Panchromatic detail.

outlines of the forms. Figure 12 shows the heavily crackled paint film discussed earlier. Most significantly it also indicates that the painted outlines cover and extend down and between the separations of the crackled ground. It is logical, assuming a common date for the ground and paint film, that the latter should be interrupted at all points of separation and that failing in this we

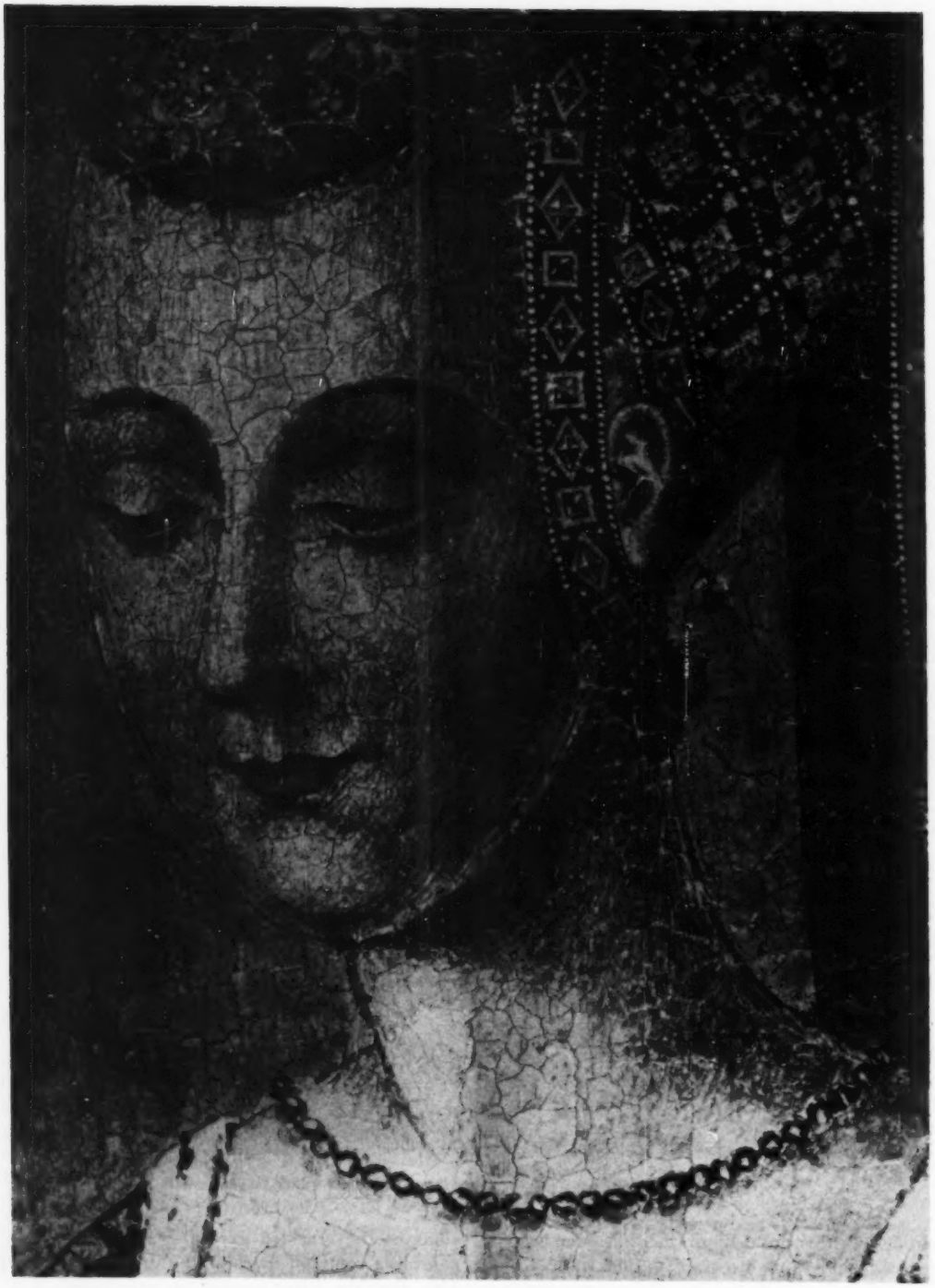

FIG. 11. *Portrait of a Young Lady of Sixteen.* Panchromatic detail.

FIG. 12. *Portrait of a Young Lady of Sixteen.* Panchromatic detail.

must conclude that the pigments were spread over a previously crackled surface.

We have assembled a number of facts concerning the panel under discussion:

(*a*) It bears a strange resemblance to the picture in the Stoclet Collection.

(*b*) Its drawing and modeling follows that found in tapestries of the fifteenth and early sixteenth centuries.

(*c*) Its *gesso* ground bears evidence of repairs pre-dating the painted surface.

(*d*) The crackle pattern exists within the body of the *gesso* ground instead of residing within the plane of the paint film.

(*e*) The system of hatching in the flesh is weak and without functional meaning.

(*f*) The paint covers the crackle, filling its separations contrary to logic.

From the above observations we are justified in concluding that the *Portrait of a Young Lady of Sixteen* is not an authentic painting of the mid-fifteenth century but an imitation of such a work and doubtless contrived as a deception.[16] We have

[16] The physical make-up of this panel is difficult to state precisely. We are indebted to Rutherford J. Gettens of Harvard University for the following information resulting from microchemical analysis of specimens taken from the gold ground, the pigment and the ground. The gold is not continuous in structure but under magnification appears as tiny flakes quite homogeneous in size. It definitely appears to have been applied as a gold powder and not as gold leaf. The pigments are extreme'y thin, finely divided and homogeneous and, despite their thinness, they seem to possess high tinctorial powers. The

not had an opportunity of examining the alleged portrait of *Isabeau de Bavière* in the Stoclet Collection and consequently are not prepared to state an opinion on its origin.

DAUMIER'S TECHNIQUE

Our next problem in authenticity concerns one of the numerous versions of the *Third Class Carriage* [17] by Honoré Daumier. The subject is reproduced (fig. 13) as recorded by panchromatic film and as seen on infra-red sensitive film (fig. 14). We begin our case by stating that the picture under study is not an autograph work of Daumier although it has appeared in monographs along with authentic pictures of this artist and has been accepted as his work by some critics. To prove this contention let us compare the questioned picture with known and universally accepted works of Daumier and see whether, on the one hand, we can discover this artist's method of painting and, on the other, whether our doubtful picture fits into the Daumier work-pattern in any acceptable sense.

The first picture we shall use as a standard of comparison is the celebrated *Third Class Carriage* [18] belonging to the Metropolitan Museum of Art (figs. 15 and 16). This masterpiece of Daumier's art, because of its unfinished state, offers a unique opportunity to examine the artist's method of drawing and painting with the opaque oil medium: a medium as foreign to his temperament as it was strange to his training and experience—a medium which he never quite understood and failed in the end to master.[19] An infra-red photograph of the Metropolitan's *Third Class Carriage* (fig. 16) fully demonstrates Daumier's approach to oil painting and his method of building pictures over a carefully drawn and functional framework of lines. During the years prior to 1848 he had worked exclusively as a draftsman in black and white, in water color and in lithography, and his works in these fields were unique in their day as masterpieces of graphic art —deep in meaning, shrewd in observation and unequalled for their knowledge of the anatomy of facial expression, gesture and pantomime. But when this great master aspired to be a painter he found to his distress that oil paint is opaque and that in the course of its manipulation his carefully drawn outlines vanished. He tried to overcome the handicap by using transparent glazes, by redrawing whenever necessary and thus slowly building solid forms in terms of paint alone. It was a difficult and indirect method and explains the large number of unfinished pictures we associate with Daumier's name—pictures in which the basic structure of outlines is still visible and which many imitators have accepted as finished works without realizing that the structural lines were employed by Daumier only as a means to an end and not as adjuncts to his final expression.

This all-important fact is emphasized by examining figures 17 and 18. The first is a panchromatic detail of the central figure of the Metropolitan's *Third Class Carriage* while the second shows the same detail by infra-red photography. In the panchromatic reproduction the structural lines are seen partially covered by thin glazes in the process of converting a linear expression into a plastic reality. Here and there, in the head, the arms, the basket, outlines have been preserved as points of future reference. The infra-red detail (fig. 18) exposes the entire fabric of structural lines as it existed prior to glazing and indicates to us that the glazing at this stage was relatively thin. We should further notice in figure 18 that Daumier's drawn indications of the figures are not confined to the outer edges of major forms but extend within these to define and control details such as hands and their fingers, the position, size, and slope of nose, eyes, mouth, cheekbones, and the disposition of the drapery. In details from the

ground is divided into three distinct layers, i.e., the lowest, consisting of calcium sulphate and glue (*gesso*), then a yellowish layer of lead white and finally a dead-white layer of lead white, both being bonded with glue. The two upper layers are firmly attached whereas they cleave quite readily from the lower layer of *gesso*, a fact which suggests that they were applied at a later time. In one cross-section through the lead white layers, Mr. Gettens found particles of decayed wood suggesting that the panel "was already old when the lead white was applied and that it was in such a decayed condition that particles from its surface could easily be swept into the lead white." Mr. Gettens concludes, "Although none of the pigments or other materials in this picture can be definitely stated to be anachronistic to the period which the painting is said to represent, they are in general much more finely divided and homogeneous than one would expect to find for that time. . . ."

[17] Fuchs, Eduard, *Der Maler Daumier*, no. 283, pl. 283, Munich, Langen, 1930. The painting is oil on canvas, H. 12⅝" × W. 16⅛". Formerly in the H. Fiquet collection, Paris.

[18] Oil on canvas, H. 25¾" × W. 35½". For a bibliography on this picture see no. 5, exhibition catalogue *Daumier*, Philadelphia, Pennsylvania Museum of Art, 1937.

[19] For a discussion of Daumier's technique see Marceau and Rosen, 1940; also the exhibition catalogue *Daumier*, 11–17, 1937.

FIG. 13. Imitator of Daumier: *Third Class Carriage.* Panchromatic photograph.

FIG. 14. Imitator of Daumier: *Third Class Carriage.* Infra-red photograph.

FIG. 15. Honoré Daumier: *Third Class Carriage.* Panchromatic photograph.

FIG. 16. Honoré Daumier: *Third Class Carriage.* Infra-red photograph.

FIG. 17. Honoré Daumier: *Third Class Carriage.* Panchromatic detail.

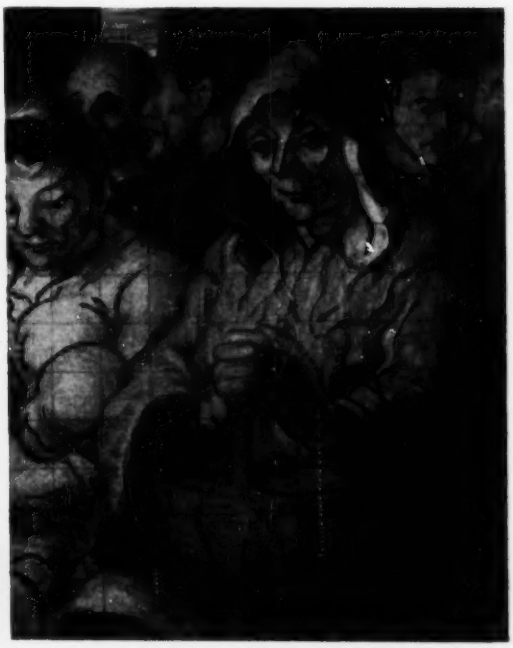

FIG. 18. Honoré Daumier: *Third Class Carriage.* Infra-red detail.

water color drawings *La chanson à boire*[20] (fig. 19)[21] we observe the same strong linear skeleton to define and place all elements in the design. In this case, however, Daumier is working in the transparent and friendly medium of water color and his washes serve to develop and realize forms without destroying the meaning and power of the lines. It is obvious that the artist is master of this medium and that he has invented a logical, effective and highly personal pictorial convention.

Before comparing our questioned picture with authentic examples discussed above, let us examine two additional oil paintings by Daumier. One of these (fig. 20) is a detail from the famous *Le malade imaginaire*[22] in which Daumier has employed heavy outlines to confine the head of the doctor's assistant. These lines are actually laid over the glazed tones of the background and are heightened here and there by lighter washes to produce an extraordinary illusion of reality and drama. But this device, however effective, was only a state or stage in the development of the finished picture —it was Daumier's method of recapturing and restating forms lost beneath layers of paint. In the *Head of a Woman*[23] (fig. 21) Daumier has made use of an old painted canvas, has resized its surface and has drawn the outlines of a new composition. Our cross-light photograph indicates that these are *structural lines*—lines to guide and to control the glazes to come. In other words we stand before the "working drawing" for a painting in which Daumier has declared his intentions in broad terms but which, for reasons unknown to us, he was unwilling or unable to carry beyond the preliminary stage. The *Head of a Woman* may not rank at the top of Daumier's production as a work of art but as a document illuminating his method it is of prime importance.

Figures 13 and 14 mentioned earlier are panchromatic and infra-red photographs of the questioned *Third Class Carriage*. Here we are dealing with an entirely different technique. These photographs are basically the same, the latter revealing none of the tentative, structural framework found in Daumier's autograph works. The painter of this picture has used thick, opaque pigment and has

[20] Fuchs, *op cit.*, no. 216, pl. 216.
[21] *Ibid.*, no. 199b, pl. 199*b*.
[22] *Ibid.*, no. 128, pl. 128.
[23] *Ibid.*, no. 10, pl. 10.

drawn with it directly on the canvas. The broad masses and weights have been stated in the painter's language of contrasting tonal values, and the areas of pigment serve as ends in themselves to suggest the drawing of the heads, hands and the shapes of the figures. Having covered the canvas in this manner the artist returned to the profiles and accented these with the dark painted outlines so characteristic of Daumier's system. These lines, however, lack structural meaning because they do not extend "within" the forms but merely "outline" them. Thus the heads and hands remain rather meaningless applications of pigment without character and without inner living form. This is true of the hands of the two principal figures where the artist has failed to resolve the question of scale and has evaded difficult problems in drawing, problems which Daumier handled with knowledge and precision in his *Third Class Carriage*. Figure 14 is especially effective in separating the "painted" and the "drawn" portions of our questioned picture and in showing that these are structurally unrelated and that the outlines add nothing of value beyond their questionable use as decoration.

X-ray shadowgraphs of paintings by Daumier fall into two main groups. In the first we may place those pictures where, because of the thinness of the glazes and the absence of dense pigments such as lead white, the shadows are faint or totally absent. The second group includes works in which dense pigments have been used or glazes have been sufficiently built up to intercept X-rays. The finished pictures fall in this category with few exceptions. One such is the unfinished *Head of a Woman*, the shadowgraph of which (fig. 22) shows an earlier and typical Daumier composition of spectators sitting in a theatre box. The light areas of the shadowgraph represent a succession of heads, and it is apparent from their indistinct outlines that much shifting and repainting took

FIG. 19. Honoré Daumier: *La chanson à boire*. Panchromatic detail.

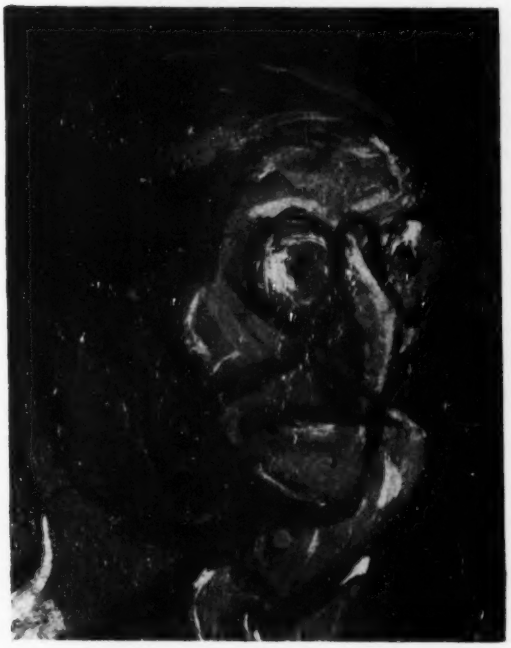

FIG. 20. Honoré Daumier: *Le malade imaginaire.* Panchromatic detail.

FIG. 21. Honoré Daumier: *Head of a Woman.* Cross-light photograph.

place before the composition was abandoned and the canvas prepared for reuse. We must also note that the outlines of the *Head of a Woman* are not recorded because of their thinness and low density. It is interesting to mention in passing that the *Head of a Woman* was at one time questioned as a work by Daumier, a view no longer held on the basis of the evidence furnished by our shadowgraph. Figure 23, typical of his finished or nearly finished works, represents *Le malade imaginaire* previously discussed in part. This shadowgraph records the thin glazes in the regions of the costumes, the shifting and redrawing of the hands and the changes in outline and position of the heads. Everything we have observed earlier regarding Daumier's oil technique is confirmed in this remarkable document of a great artist's struggle to master a strange and stubborn medium. The figure of the doctor's assistant, at the extreme left of the picture, does not register here because, as we have seen, it is a fleeting ghost figure of drawn lines over dark glazes, not yet given its final position in space.

Let us now study the X-ray of our questioned *Third Class Carriage* (fig. 24), bearing in mind the general appearance of shadowgraphs of authentic pictures by Daumier. In the X-ray before us we sense a state of confusion and lack of continuity in the drawing of the figures and detect two distinct sets of shadows—those cast by the principal figures of the *Third Class Carriage,* and, beneath these, shadows of forms suggesting a landscape. Thus, at the right, there is a gable-like structure and a sloping tree trunk, indicating traces of another composition under the visible paint film. It is further apparent that this earlier picture has been scraped to remove heavy pigment along the lower part of the canvas and in the section occupied by the seated figures. We have stated that at times artists resort to the practice of using a canvas more than once but in such cases of re-use the superimposed pictures maintain their identity as works by the same intellect and hand. The present example is not in this class and it becomes certain that we are dealing with an imitation in which an old foundation has been used to give a semblance of age and authority to a feeble effort. Daumier sought to express forms by trial and error, by drawing and redrawing. This was observed in the X-ray of the *Malade Imaginaire*

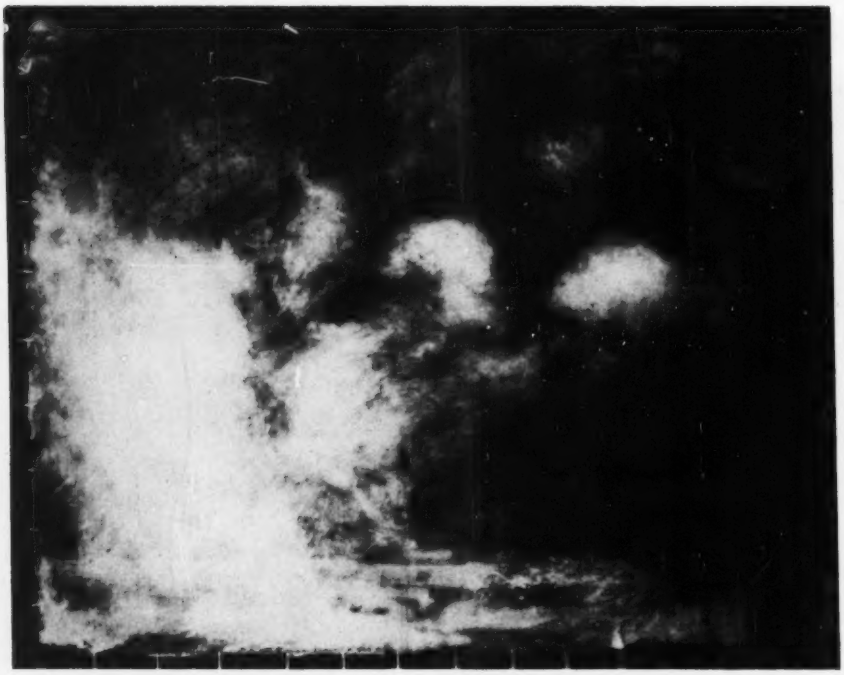

FIG. 22. Honoré Daumier: *Head of a Woman.* X-ray.

FIG. 23. Honoré Daumier: *Le malade imaginaire.* X-ray.

FIG. 24. Imitator of Daumier: *Third Class Carriage*. X-ray.

where the heads register as blurred light areas, a condition which does not exist in the present shadowgraph. Instead, shadows of the heads read in the X-ray exactly as they do in the panchromatic photograph and Daumier's method of drawing with paint cannot be recognized. Since we have also seen that the infra-red photographs do not correspond to Daumier's work-pattern, we are able to reject the picture as an autograph work by that artist.

Now examine two additional works signed with the familiar *h D*, a signature to be found on most works by Daumier and on many with which he had no connection. It will hardly be necessary in these cases to burden our discussion with a multitude of illustrations because the surface appearance in each case is significant.

The first of these pictures [24] is a small canvas known as *The Barker* (fig. 25). Closely related to a larger and more elaborate work entitled *The Mountebanks*,[25] it is doubtless the picture referred to by Eduard Fuchs [26] as follows: *"Von diesem Bild gibt es eine fast identische kleinere Wiederholung."* It has not been possible to examine the larger picture, once owned by the Baron Vitta in Paris, but from the reproduction published by Fuchs the two pictures are strikingly similar, not only in composition but also in drawing and in the distribution of the lights and darks of the modeling. Figure 26 reproduces the X-ray shadowgraph of *The Barker* and shows the picture to have been painted over an earlier portrait of a man with strangely oriental features—a performance quite foreign to Daumier's method and entirely unknown in his work as a type. It could be argued that Daumier possessed an old portrait of this kind and reused the canvas. Assuming this to be so; we must still reconcile the drawing of the picture before us with our knowledge of Daumier's practice and, above all, with our understanding of his structural use of drawn outlines. Figure 25 shows outlines used only in the profiles of the figures or to indicate the contours of drapery. They actually have little function since, in the fig-

[24] In this case, as with the other controversial items discussed here, owners' names and sizes have been omitted purposely.

[25] Fuchs, *op. cit.*, no. 125, pl. 125.

[26] *Ibid.*, 50, no. 125.

FIG. 25. Imitator of Daumier: *The Barker.* Panchromatic photograph.

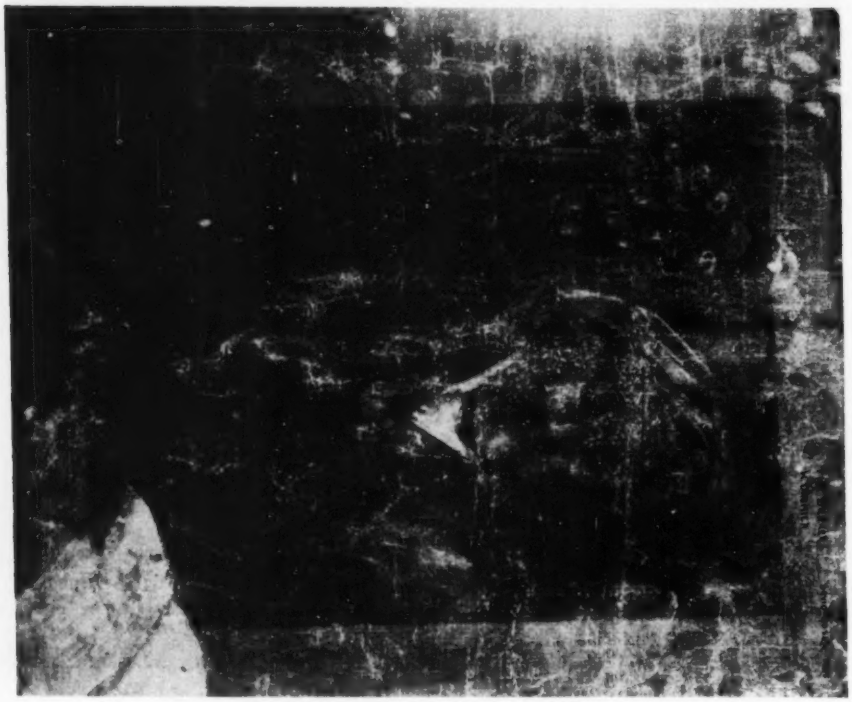

FIG. 26. Imitator of Daumier: *The Barker.* X-ray.

ure of the clown, they are not followed in delineating light and dark areas but merely decorate the surface of the canvas at random. The author of this work has employed broad, sweeping strokes unlike those we associate with Daumier's meticulous and searching brush. Further points at variance with Daumier's technique might be cited and would include the lack of drawing in the heads, the spotting-in of eyes with formless accents of paint, the shapeless, boneless hands, and the artist's preoccupation with the surface appearance of forms rather than with their basic structure. These serious faults betray lack of observation and knowledge and are unworthy of Daumier as we know him even in his most casual moments.

The second painting (fig. 28), also signed *h D*, was discovered a few years ago among the effects left by an aged artist who had spent his student years in Paris at the turn of the century. The composition, unknown among Daumier's oil paintings, is the subject of one of his lithographs entitled *Les envies de madame*[27] (fig. 27). It will be observed that the figures in both pictures are

FIG. 27. Honoré Daumier: *Les envies de madame.* Lithograph.

[27] This was published in La Caricature for March 7, 1841, as plate 32 in the series *Mœurs conjugales;* Delteil, Loys, *Peintre, Graveur Illustré* **20–29** (Honoré Daumier): no. 655, Paris, chez l'auteur, 1925–1930.

FIG. 28. Imitator of Daumier: *Les envies de madame.* Panchromatic photograph.

shown at the same scale, the disposition of all major and minor details in the oil painting correspond exactly with their counterparts in the lithograph, and the figures in both face in the same sense. We know that the lithograph was published March 7, 1841, and may have been made somewhat earlier. We know also that Daumier's first experiments with the oil medium date from the neighborhood of 1848 when he painted his sketch for *La République* now owned by the Louvre. Hence it would seem that our painting, if by Daumier, must date after 1848 and that the lithograph served as its model because both pictures have the same orientation or sense.[28]

Further study of our lithograph and painting discloses important facts. The oil painting is a

[28] It is a well known fact that a design drawn on lithographic stone will, when printed, appear reversed in the print. It is also known that in the making of reproductive prints of paintings or of topographical subjects, where it is essential to reproduce the true orientation of the original, engravers and artists work in reverse on the plate or stone. The creative artist will occasionally base a print on the subject matter of an oil painting. When this happens, the sense or orientation of design on the stone or plate will correspond to the sense of the painting but the resulting print will appear reversed.

FIG. 29. Imitator of Corot: *Haydée*. Panchromatic photograph.

precise, line for line, repetition of the lithograph—a practice we cannot find duplicated in Daumier's work and one foreign to any creative spirit. Many examples of this might be cited but perhaps two will suffice. In 1864 Daumier published his lithograph *Croquis dramatique* No. 2.[29] Shortly thereafter he painted a canvas *Le drame*.[30] In the latter he followed the main theme of his lithograph but rearranged spacial relationships and generally revised details. Similarly, in 1862 Daumier's lithograph *L'Âne et les deux voleurs*[31] appeared and was followed by a painted variant.[32] Here again Daumier did not copy the lithograph but interpreted it freely, by shifting the placement of figures on the canvas, modifying gestures and details of anatomy. These examples furnish a clue to Daumier's practice when he repeated an old theme in the oil medium. It follows that we must look with suspicion upon works copied too exactly from earlier prototypes.

[29] Delteil, *op. cit.*, no. 3280.
[30] Fuchs, *op. cit.*, 45, repr. facing 48.
[31] Delteil, *op. cit.*, no. 3253.
[32] Fuchs, *op cit.*, 51, no. 136, pl. 136.

Fortunately, in the case of the oil version of *Les envies de madame*, stylistic considerations point conclusively to the fact that we are dealing with a copy. The drawing of the figures, for instance, is done directly in terms of paint, light areas contrasting with dark to express the forms. Daumier's careful building of masses by placing glazes over drawn outlines is absent; the brush strokes are heavily loaded with pigment, applied rapidly and broadly with little concern for structure or solidity of form. The artist has imitated the surface appearance of Daumier's lumpy paint without understanding the functional reason for its existence.

A COPY AFTER COROT

The next problem in authenticity concerns one of Camille Corot's charming figure pieces and an old copy of the same subject. The case is perfect for our purposes because it proves conclusively what has been said about the hand and mentality of the creative artist and that of the copyist. The original picture was painted between the years 1870 and 1872 from Corot's favorite model, Emma

FIG. 30. Corot: *Haydée*. Photograph reproduced by Alfred Robaut.

Dobigny. It is reproduced in Robaut's [33] definitive catalogue under the title of *Haydée* with the notation that it was sold by Corot to M. Gandouin and then passed through the Perreau Collection and in 1887 was owned by M. Goupil. Robaut also significantly notes "il existe une copie de ce tableau." After 1887 the picture appears to have belonged to M. Dieulafoy, and because it was not publicly exhibited its existence was more or less forgotten until 1930 when it was shown at the Galerie Rosenberg [34] in Paris. It reappeared in exhibitions in Zürich,[35] 1934, in Paris,[36] 1936, both times listed as the property of the Baron Napoléon Gourgaud, Paris. Figure 30 shows the picture as reproduced by Robaut. Since this illustration is identical with the reproductions in the catalogues of the above mentioned exhibitions, there can be no doubt that the painting owned by the Baron Gourgaud in 1936 is the one known to and certified by Robaut as an original Corot.

Within the last twenty-five years a second picture entitled *Haydée* and signed COROT appeared on the market and was purchased by a private collector. Figure 29 is a panchromatic photograph of this work which is remarkable for its apparent duplication of every detail of the original. The question before us is to determine whether this second version of *Haydée* is a copy, perhaps the one referred to by Robaut, or a replica from the brush of Corot. To this end let us examine photographs of a number of unquestioned works by Corot and become familiar with his technique.

Figure 31, an X-ray shadowgraph of Corot's *Gipsy Girl at the Fountain*,[37] reveals the substructure of the artist's painted surfaces and his method of building forms. It will be noted that the modeling is realized by an orderly progression from the darkest areas in the shadows to the lightest portions. This method is found in the head and hands and can also be observed in the drawing of the drapery. It illustrates what Corot meant when he said, "Je commence toujours par les ombres, et c'est logique; car, comme c'est ce qui vous frappe le plus, c'est aussi ce que l'on doit rendre d'abord. Ce qu'il y a à voir en peinture, ou plutôt ce que je cherche, c'est la forme, l'ensemble, la valeur des tons." [38]

Corot's words and our shadowgraph of the *Gipsy Girl at the Fountain* will enable us to appraise the shadowgraph (fig. 32) of the copy under study. In this, Corot's orderly development of forms is absent, the dark and light areas of the modeling lack proper definition and they fail in their functional purpose. We sense disorder throughout and can see no volumes or forms in the making, but instead a distribution of accents of equal value in all directions. It is apparent in these two shadowgraphs that the brute substance of paint has been applied differently: one method is logical and unified and the other illogical and confused.

Details of both pictures further illustrate the divergence in methods used by Corot and his copyist. Compare the sparkle of the small brush strokes in figure 33 with the flat paint seen in figure 34. These photomacrographs record different handwritings, one of which (fig. 33) is direct and expressive while the other is halting and evasive. The modeling from dark to light in figure 33 carries through logically, and the resulting forms are bone and sinew and alive. In figure 35 the same condition exists: crisp hatchings merge shadowed portions with those in full light, the ordered use of contrasting tones create excitement and surprise in every passage. This masterly handwriting is Corot's and has nothing in common with the washed-in and glazed paint in figure 36.

Two additional comparisons should suffice as a control in our chain of reasoning. These are furnished by figure 37, *La dame en bleu* [39] of the Louvre, and *Mother and Child* [40] of the John G. Johnson Collection (fig. 38). The technique in both is clearly related to the artist who painted the *Gipsy Girl at the Fountain* although in each case the problems of format, lighting, and composition are totally different. Thus it is clear that an artist's method is consistent and personal. If this has been shown adequately, the artist of our questioned *Haydée* was someone other than Corot: an artist whose preoccupation was with the surface appearance of paint and not with the true structure of living forms.

The several problems discussed have illustrated the practical uses of photographic aids in the study of paintings. We have seen that photography

[33] Robaut, Alfred, and Étienne Moreau-Nélaton, *L'œuvre de Corot*, no. 1997, Paris, Floury, 1905.
[34] *Corot, paysages de France et figures*, no. 51, Paris, Galerie Rosenberg, 1930.
[35] *Corot*, no. 114, Zürich, Kunsthaus, 1934.
[36] *Corot*, no. 95, Paris, Musée de l'Orangerie, 1936.
[37] Robaut, *op. cit.*, no. 1423.
[38] *Ibid.* **1**, 260.
[39] *Ibid.*, no. 2180.
[40] *Ibid.*, no. 1385.

FIG. 32. Imitator of Corot: *Haydée*. X-ray.

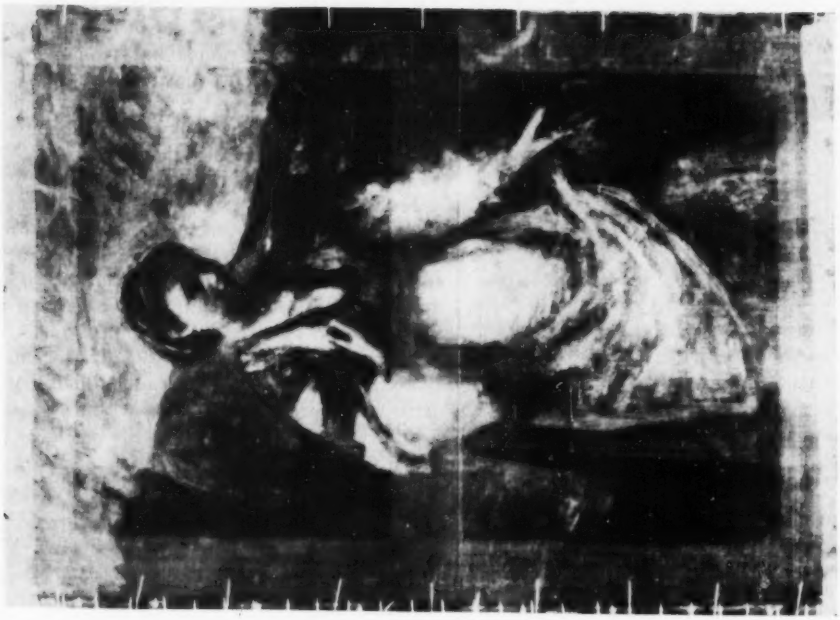

FIG. 31. Corot: *Gipsy Girl at the Fountain*. X-ray.

FIG. 34. Imitator of Corot: *Haydée*. Panchromatic detail.

FIG. 33. Corot: *Gipsy Girl at the Fountain*. Panchromatic detail.

FIG. 36. Imitator of Corot: *Haydée*. Panchromatic detail.

FIG. 35. Corot: *Gipsy Girl at the Fountain*. Panchromatic detail.

FIG. 38. Corot: *Mother and Child.* Panchromatic detail.

FIG. 37. Corot: *La dame en bleu.* Panchromatic detail.

greatly extends our normal vision by exposing important facts concerning the substructure of paintings. If a sufficient number of photographic documents are assembled on a given problem of authorship or authenticity, and related through comparison with similar material drawn from unquestioned works, the problem can be resolved on stylistic grounds with a fair degree of accuracy. It must be emphasized, however, that laboratory aids can never entirely replace historical research,[41] and that both approaches must be fully explored before our conclusions can become universally valid.

[41] Rawlins, F. Ian G., Evidence: Its nature and place in the scientific examination of pictures, *Tech. Stud.* **8**: 75–81, 1939–1940.

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